Problems of Science

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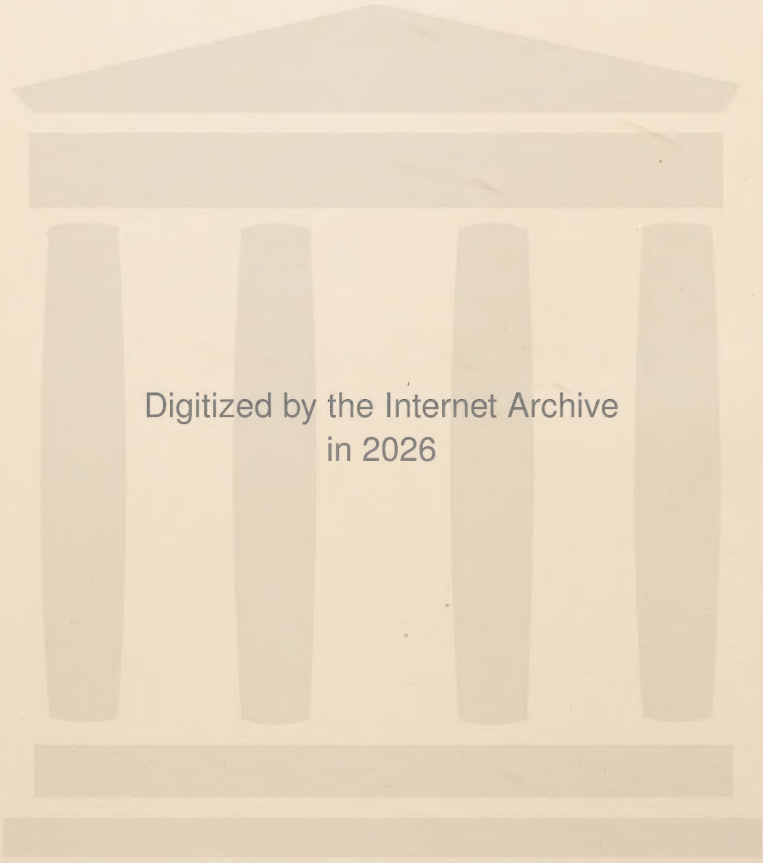
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PROBLEMS OF SCIENCE

BY

FEDERIGO ENRIQUES

AUTHORIZED TRANSLATION BY
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WITH AN INTRODUCTORY NOTE BY
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INTRODUCTORY NOTE.

THE various causes which have so long delayed the publication of the present translation of the *Problems of Science,* may prove to be rather aids than hindrances to the just appreciation of this very remarkable synthetic view of scientific methodology. For, as a result of these accidents, the book of Professor Enriques is offered to American readers at a time when we are better fitted than we have been during the last few years to appreciate the significance of the author's large, clear, and calm view of a wide range of perplexing modern problems.

The first edition of the Italian text of the *Problemi della Scienza* of Professor Enriques appeared in 1906, and had already become known to a wide circle of European students, belonging to various nationalities, at the time of the International Congress of Philosophy at Heidelberg, in the late summer of 1908. At this congress I myself met the author, and undertook to do what I could towards finding an American publisher for a translation of this book. Not long after the congress, Dr. Carus, on behalf of the Open Court Publishing Company, agreed to undertake the publication of the translation. The translator completed the first draft of the manuscript by June 1909. A certain amount of revision of some of the more technical portions of the translated text remained as that part of the work which I had myself, from the outset, agreed to undertake. Moderate in quantity as this task of revision has indeed proved, it came into conflict with a great number of academic and personal duties of my own,—duties which resulted from my previous engagements, and which could not at once be laid aside for the purpose of finishing my own little part of the task. Various new hindrances later intervened. In consequence of my own delay, the revised manuscript of this translation was first put in the publishers' charge as late as June 1912; and this American edition of the work of Enriques has since been in press. The delay has given oppor-

tunity to use the second Italian edition of the *Problemi* for the purpose of the revision of some passages of the translation.

Since the Heidelberg Congress of Philosophy in 1908, pragmatism, which, as many readers of current discussion will remember, formed the principal topic of the lively discussions of that session, has passed through its days of joyously youthful success; and is now no longer a novelty. Meanwhile, the new star of Bergson has glowed with increasing brilliancy from year to year. "Anti-intellectualism" has become, for the time, the prevailing mood in the more popular expositions of philosophy. Mobile minds,—minds characterized by what James called a "dramatic" temper,—have taken a leading part in controversy. Books such as the present one may seem for the moment, to such minds, out of place.

Yet precisely such moods as have been so widely represented in the general literature of popular philosophy since 1908, call for their own correction, or at all events for their own complement and supplement. What is most to be feared, at a time when discussion is so lively and when "anti-intellectualism" has gained such large and eager audiences, is not any definitive triumph of the "anti-intellectual" enthusiasms, but rather some too swift and "dramatic" reaction in the world of the ruling philosophical interests, some drastic return from the revolutionary temper of the thought of the moment to the older types of scientific orthodoxy, some renewal of the "dogmatic slumber" from which James, the Pragmatists, and Bergson, have awakened many plastic, quick-witted, but not always naturally judicial minds.

At just such a moment, a book like the present work may therefore be especially useful to thoughtful students, who love patience and clear ideas quite as much as they are fond of intuitions, of brilliancy, and of "vital impetus." The work of Professor Enriques stands somewhat above and apart from those philosophical controversies which the anti-intellectual movement has inspired; for this book was prepared and published in the original Italian before those controversies assumed their latest phase. Yet the author, already prominent in the discussions of the Heidelberg Congress of 1908, has since been President of the Philosophical Congress at Bologna in 1911. Translations of his *Problemi della Scienza* into French and German have widely extended his influence. His book is by far the most thorough and synthetic treatment of the problems of scientific methodology which belongs to recent years,—with the sole exception of the treatment which forms part of the first two

volumes of Merz's *History of Thought in the Nineteenth Century.* Meanwhile, owing to their widely contrasting ranges and modes of discussion, Merz's book, (which is primarily a history of science, with a treatment of methodology *obligato*), and the book of Enriques, (which is explicitly a scientific methodology, with numerous references to contemporary interests and controversies):—these two books, I say, come into no sort of rivalry with each other, but supplement each other in a way which is all the more important because neither author can have known, I think, about the other's work until his own was substantially complete.

As for the relations of the book of Enriques to the recent controversies to which I have just referred, the work on the *Problems of Science* is thoroughly "intellectual" in its tone and temper, without being open to any of the usual objections to "intellectualism" which are now most popular among philosophical readers. The author (himself Professor of Projective and Descriptive Geometry in the University of Bologna), approaches his "Problems" with the training of the mathematician and the logician, and with the reputation which his treatise on "Projective Geometry," and his published essays on the "Foundations of Geometry" have long since won for him. Yet this book shows no tendency to magnify overmuch the office of the geometer, or the authority of the logician, or the powers of the human reason, in the interpretation of phenomena. Pragmatists will find Enriques emphasizing some of their own theses regarding what is now called the "instrumental" or the "functional" significance of thought, and of the whole scientific process. And this emphasis, as it appears in some of the most important general discussions (notably in the latter half of the chapter on Logic), is all the more interesting because (as we have just seen) this book,—especially in its earlier chapters,—antedates the most recent developments of pragmatism. Yet this relatively pragmatistic element of the book of Enriques appears in a form which is both largely original, and extremely many-sided and judicial. Enriques views the thinking-process as indeed an "adjustment" to "situations." But he lays great stress upon the tendency of science to seek unity, upon the synthetic aspect of scientific theory, upon what he calls the "association" of concepts and of scientific "representations." And this stress upon synthesis, this sense for wholeness and for unity, gives his treatment both of the values and of the limits of scientific hypotheses and theories, an original and a very notable character. In his view of the work and of the

uses of natural science, Enriques stands in strong contrast to the original or Comtean type of "Positivism"; for he greatly emphasizes both the "objective aspect" and the significance of constructive scientific theories. As a methodologist, Enriques also finds a positive value in many "hypotheses" of such a type that Ostwald's well-known maxims of scientific method would condemn them in advance. Nor does Enriques agree with Mach's or with Pearson's limitation of the business of science to the simple "description" of physical phenomena.

Yet, despite this fondness and this respect for synthesis and for the "association" of various scientific concepts and "modes of representation," Enriques has as sincere an aversion to what he takes to be genuinely "metaphysical" constructions as has any positivist; as vigorous a hostility to the "transcendental" and to the "absolute" as is cultivated by any philosopher of our "Chicago School"; and as clear, if not as vehement, a respect for the relation between thought and will as is expressed by any Pragmatist.

What sets Enriques most apart from most of the thinkers,—pragmatists, positivists, relativists,—with whom one would be most likely to associate him,—or on occasion to confound him,—is a certain judicial temper, a breadth of view, a fondness for synthesis, an exactness of intellectual training, a love of the comparative study of his topic,—in brief a spirit which is as rare as it is requisite in a man who is to prove a thoroughly good methodologist. Enriques certainly does not, as a philosopher, blindly overrate the work or the powers of the intellect. On the contrary, he emphasizes the imperfection, the relativity, the tentative and inadequate character of all scientific and theoretical construction. Yet he is neither sceptic, nor anti-intellectualist. He does justice to the "instrumental" function of thought. But he is certainly no mere "instrumentalist." For the stress which he lays upon the "objective aspect" of even the most highly theoretical portions of scientific theory; and his insistence upon the tendency of science towards a genuine and irrevocable progress, not merely in its mutable and transient control of special experiences, but in its total view of nature,—these tendencies in Enriques seem to exclude any interpretation of his philosophy of science as a mere "instrumentalism." For Enriques, the "absolute" is no object for science. But what is won, in a scientific way, is won, and the whole tendency of the scientific attainment of truth is to be *not* a dealing with what is merely mutable, but an irreversible progress towards a survey of the unity of the real,—

a grasping of real "invariants," and of wholes. These are theses that have a prominent place in the extremely careful, far-seeing, critical, and constructive methodology which constitutes this wealthy and well-wrought book.

Where so much is offered, it is hard to select what the reader should most consider. Personally I have taken very special interest in the treatment which Professor Enriques gives to the Principles of Geometry,—a topic which he has made especially his own, and which (as here discussed) will appeal not only to students of the logic of mathematics, but to psychologists interested in those aspects of the problem of space which especially concern their own work. The concluding chapter, dealing as it does with a wide range of highly technical physical problems and theories, is at once the most difficult (both for the translator and for the reader) and the most characteristic of the book. Here the synthetic tendencies of our author,—his wide outlook, his fairness of judgment, his careful comparisons, his bringing together of matters which are, for most readers, hopelessly far apart, — all tend to show what this book is, — a treatise on methodology such as we have long needed, and have here at length before us in English. May the work of the President of the last Congress of Philosophy serve to quicken as well as to nourish interest both in science and in methodology. May it aid us in treating more judiciously, more broadly, and more exactly, the current controversies concerning the office and the scope of the human intellect. And above all may it foster that spirit of unity in thoughtful research which its author has so well illustrated,—that spirit namely which tends to unite the work, not only of various sciences, but of various nations.

December, 1, 1913.

Josiah Royce.

PREFACE.

A TRAIN of thought which gradually came to maturity during the ten years from 1890 to 1900 has resulted in a critical study of certain problems relating to the logical and psychological development of scientific knowledge. These problems are here entitled: "Problems of Science."

The plan of the work may be said to have been settled, with the exception of the last chapter, ever since the year 1901. In that year I began to state my views of the subject in various lectures and conferences. The formal arrangement of the material has been but slightly retouched since that time.

It is rather difficult to state how the general spirit of my treatment is related to the philosophical distinctions current in the schools. I should like to characterize this spirit as at once *critical and positive*: because I really think that I have interpreted in a clearer and more scientific way, and have reconciled (without eclectic compromises) certain speculative tendencies by which my thought was prompted at the outset. But I do not conceal from myself the fact that the ideas set forth in this book are profoundly different from those which are current under the name of *critical positivism*. The reading of the first, or introductory, chapter alone is sufficient to prove this fact.

The arguments that are developed follow the headings of the various parts of the work, which are recapitulated in the index. The connection between these widely different topics consists in a general view of scientific procedure, which I have sought to explain by means of an inductive exposition, reenforced by many examples.

The analysis of what constitutes reality is developed in Chapter II into a critique of facts and of theories, so handled as to distinguish between the positive content of science on the one hand, and its subjective aspects on the other.

From this analysis arise two classes of problems which are

successively studied: (1) the problems connected with the logical transformation of concepts, regarded both as a psychological development and as an instrument of knowledge (Chap. III); and (2) those problems which refer to the significance and to the acquisition of the more general concepts of space, time, force, motion etc. (Chaps. IV, V).

The theoretical questions of physics are examined in Chap. VI, in connection with a critique of the theory of mechanics; and this critique ends with certain observations as to the extension of mechanical explanations into the field of the phenomena of life.

The idea of science which I have formed is not here explicitly developed in harmony with a general system of philosophy.

It is not a part of my plan to examine the relations between *cognition* and *volition* further than is required by the very definition of science. And so I postulate the value of science, and every further effort to appreciate what that value is will be excluded from this critique.

But that does not imply that I consider knowledge as an end in itself. It is plain that "science for the sake of science" is a formula devoid of social content. And on the other hand it is plain that cognition can offer to volition only the *means* to work with, but not the *ends*, and that it is absurd to seek in science the *guiding principles of life*.

But let us keep in mind that the will to be scientific, outside of its utility, offers in itself a significant moral standard. For such a will recognizes and affirms the True as independent of fear or of desire, and in this way the consciousness, as well as the power, of a will capable of looking beyond the transitory ends of the present towards a more lofty future progress, promotes the full development of the human being.

My faith in this *philosophy of science* has led me from the fields of geometry, where thought rests quietly in the security of acquired facts, to discuss the building up of a science of knowledge which may become the common possession of the studious and may tend to unify the various domains of knowledge in one synthetic view of the cognitive methods.

Hence the plan of a work, which, by bringing together so many diverse objects and problems, oversteps the boundaries of custom set by our scientific public, and which must thus excite a natural distrust.

To offset this I have merely the consciousness of my fifteen

years work. But where indeed learning and talent prove themselves too unequal to the arduous task, my mind is buoyed up by the hope that the labor will not have been in vain if the vision of the unity of science thus evoked may avail to unite in a common brotherhood, and for a lofty end, the efforts of our youth, which tend in the various lines of study to the conquest of truth.

May this vision rise above the differences of matter and of technique, as I progress. And may it above all so light the way for Italians, that the work of renewal begun by our elders may be continued through the full and harmonious development of our national genius.

F. E.

I. INTRODUCTION.

1. THE SPECIAL PROBLEMS AND GENERAL IDEAS OF SCIENCE.

A DOUBLE fatality hangs over one who has consecrated his days to science.

If he would contribute to the advancement of science, he must prepare himself first of all by a patient study of the thousands of details which constitute its technique; he must learn the results obtained by numberless laborers whose researches tend toward the same aim. He must master their conceptions and subject them to a new criticism.

This work so engrosses the attention of the investigator that he has little time left for casting a glance over the branches of science which are developing beside him. Yet this necessity also weighs upon his soul! If on the one hand he ought to study special problems, on the other, he cannot exempt himself from considering the ends set before special research by rising to a general outlook which shall command the view of a broader scientific basis.

This double necessity causes a conflict of tendencies, and this conflict in our system of production results in a loss of time and of work from which the intellectual world suffers.

Most investigators, if they are not rightly directed, shut themselves up in a narrow circle and fall into a blind empiricism. Others lose themselves in the region of confused generalities, while a few finer spirits find the way for themselves, and often must win again by fresh efforts that which they should have a right to expect as the outcome of the completed work of their companions in labor.

But the age of heroes, that of Descartes or Leibniz, whose genius opened all the doors of science, seems closed forever!

The conquests of the past weigh upon the present and upon the future. And if it is permissible to hope that a happier use of our intellectual powers may put an end to the confusion of to-day,

still it is neither credible nor desirable that we should return to a state of affairs in which science was the exclusive task of a few superior men. We must recognize that a thousand forces united may succeed in raising those masses of rock which weigh so heavily upon the shoulders of the giant.

Our need is to perfect the organization of that work which ought to be accomplished, under free conditions, through a fitting scientific education. To this end it is necessary that all enlightened men, in whatever special branch of study they are respectively working, should be conscious of the unity of the aims of science.

They will then give a helping hand each to the other, and will aid one another in cordial understanding. The isolated efforts of individuals will be replaced by the more fruitful work of learned societies.[1]

Nor will superior minds lack a place in such organizations of productive work. Rather, freed from the need of hampering their abilities as investigators by the acquisition of an over-minute learning, they will be able to profit more freely by the advantages of this community, and in consequence to make themselves more useful to it. They will become organizers, connecting the various researches to the general ends of science, of which they will then be able to gain a wider and more precise view.

But while we thus imagine the plan of an ideal organization for scientific production, are we not merely indulging in a poetic dream?

Certainly we must not conceal the difficulties which present themselves upon our road; but these cannot rob us of our faith in progress, which must indeed be slow, but which must lead, in each domain, to superior forms of social life.

The end for which we ought to strive to-day is a scientific education, which shall enable the workers in any field whatsoever to understand better how the object of their own research is subordinated to more general problems.

We need to awaken in people's minds the feeling of a larger harmony in which apparent contradictions are reconciled.

Nothing is so dangerous as to shut oneself within a circle from which, with rigid logic, everything is banished that does not fit in with the results of a narrow experience!

[1] The signs are not wanting that the need of scientific association is becoming more and more acutely felt. I will cite as authoritative testimony that of E. Picard in his admirable report upon the general condition of the sciences published by the Paris Exposition in 1900.

2. SCIENCE AND PHILOSOPHY.

The aims thus sketched are very clearly connected with the influence which philosophy ought to exert upon science. For, in sum, philosophy is the expression of an impulse which in the ordering of the various branches of knowledge, urges us, although in various ways, towards unity and generality.

But if philosophy does not accomplish its office as completely as is needful, the reason should be sought in that state of things whence arose, at the beginning of the past century, the sad strife which still divides the philosophers from the men of science. We do not here purpose to search into the causes of this strife, since it would not now be helpful to renew the old recriminations, when on either hand are now to be seen the signs of a happy reconciliation.

Let us merely observe that the judgment of students of science ought calmly to correct that lack of clearness and precision which accompanies certain nebulous expressions of speculative thought. But such a correction loses all its efficacy in case it includes in common condemnation one who only conceals the emptiness of his purposes beneath obscure language, and one who, being perhaps the victim of some unavoidable error of method, still aims to find unity in multiplicity and to separate the determinable from the indeterminate.

And the value of criticism is still more diminished, when, not satisfied with blaming the philosopher, it turns against philosophy itself and contrasts with the variability of the latter the solid constructions of the edifice of science. This accusation can be heeded only by one who has not comprehended that philosophic thought does not necessarily require the solution of particular and well-defined problems. Philosophy represents rather a *tendency* of the human intellect—a tendency which determines, so to speak, the *style* of the edifice of science, so that this edifice takes on diverse forms as it grows.

The severe judgment of scientific men, of which we have already spoken, has been most especially directed against that philosophy which, under the influence of modern tendencies, attempted to scale the loftiest summits of abstraction, during the first half of the last century.

Fortified by the decisive condemnation of Auguste Comte, positive science entertains the belief that there is in this philosophy no movement of ideas, but only a vain battle of words. Such an opin-

ion does not confine itself to combating the metaphysics of the modern systems as a vicious manner of treating certain problems, but it even goes so far as to deny the very existence of the problems referred to by such speculations.

Thus it not seldom happens that the reader of Comte, ignoring strangely enough his constructive work in his exposition of the general results of the sciences—a work the freshness of which compels admiration after sixty years—that the reader, I say, accords the larger favor to what is negative in this philosophy. For Comte metes out equal condemnation to everything that was the object of classical inquiry. To him it seems without examination that all this labor has been spent in vain, because its aim was to make known that which never will be knowable in any sense whatever.

3. THE AGNOSTIC RENUNCIATION.

It has been justly observed that the present tendency in dealing with such matters results from an especially modern attitude of mind towards certain traditional questions. We have hoped that a sort of general pacification would result, for our whole social life, from that critical or dogmatic agnosticism to which all ways of speculation led during the last century.

A little more than thirty years ago, an illustrious physiologist (Du Bois Reymond) proclaimed the concept of such an agnosticism, summing it up in an eternal *Ignorabimus* which weighs upon the science of to-day.

And more recently a movement of thought, which constitutes a singular retrogression in the history of civilization, has arisen with "the bankruptcy of science" as its war-cry; a cry purposely chosen to signify the plain principle that knowledge cannot set a standard for the will. Not in vain was aroused the phantasm of a reality which must remain forever inaccessible to any kind of scientific determination.

But it is not a part of our plan to examine here, under such an aspect, the consequences of the philosophic renunciation expressed in the affirmation of the *unknowable*.

Let it suffice to observe that a happy reaction is at work at present against this cowardice of the modern spirit, and is already clearly shown in various fields of science.

Truly those who have had the audacity to set limits to human knowledge have not always been so prudent as to keep to the vague region of indefinite things. The bounds thus set have mostly been

surpassed, in an unforeseen way, and it has become plain that it is no more legitimate to affirm our future ignorance about any subject whatever, than it is to believe ourselves possessed of a knowledge not yet acquired.

Let us cite a single instructive example, calling to mind how spectrum analysis succeeded in disproving after a few years the statement of Auguste Comte which denied that astronomy could penetrate the mystery of the chemical constitution of the heavenly bodies.

For the rest the surest proofs upon which agnostic conclusions attempt to lean, offer slight resistance to the progress of criticism; so that they may be compared to certain fortresses, terrible machines of war, which no superior force could beat down, but which a handful of men may easily overcome by making a detour, without joining battle at all.

Just so does science proceed, going around the difficulty which blocks its road!

Science would never have reached its present state, had it not been constantly changing the form and the statement of its problems, adapting the scope of its researches to the changed conditions of thought.

This kind of procedure seems so general, in every branch of the knowable, that a skeptical mind, considering things under a special aspect, might well smile at a progress which was never permitted to follow a straight line.

But since, none the less, he who thinks of things in their entirety, must recognize that there has been progress, it is plainly seen that scientific questions include something essential, apart from the special way in which they are conceived in a particular epoch by the scholars who study such problems. The search for this essential aspect which is concealed in every question, is the office of the true philosophical spirit, which is not satisfied with pausing on the surface of things.

4. THE SO-CALLED INSOLUBLE PROBLEMS.

In a broad sense there are no insoluble problems, since every problem corresponds to a feeling, often obscure, which may be satisfied by the discovery of some new fact that increases our power over the external world. There are only problems not yet suitably expressed, and idle discussions, void of sense, in which through defect of method, we at times wander far from our actual goal, be-

cause we do not know how to set it clearly before us, and so are seeking an answer to ill formulated questions.

This is what the history of science clearly shows us, and from that history we will try to draw some instructive examples.

5. THE SQUARING OF THE CIRCLE.

One need not be versed in geometry, mechanics or chemistry, to have heard of certain famous problems, such as the squaring of the circle, perpetual motion, and that changing of metals into gold, for the sake of which medieval investigators so wearied themselves in their search for the philosopher's stone.

Such problems, which are commonly quoted to prove the weakness of the human mind humbled before insurmountable difficulties, offer food for more interesting reflections, by which our faith in scientific thought may be confirmed.

The problem of squaring the circle is the most noted of the three enigmas that the Greek geometricians have left as an heirloom to their successors.

The trisection of the angle and the duplication of the cube had already received, in modern times, a sufficient solution, but the squaring of the circle completely defied the principal analysts of the past century. Only twenty-four year ago (1882) this difficulty was conquered! But the *manner* in which such a result was reached, and the very *sense* of the solution obtained, have the greatest interest in relation to our purpose.

"To square the circle" means, for one who has not an exact notion of the problem, "*To construct* a square containing the same area as a specified circle." That such a square *exists,* reasons of continuity may easily demonstrate, since the side of the square itself may be easily constructed, when we have a segment equal to the length of the circumference.

This observation suffices to assure us that the proposed problem is not absolutely *impossible.* Yet all efforts, renewed almost without pause during twenty centuries, have of necessity been doomed to come to naught in face of the *insufficiency of the means* which were expected to do the work.

Nor would any intellectual superiority have given the key to the enigma, had not a new critical method cleared up the old concepts relating to the *solution* of geometric problems.

The *ruler* and the *compass* were the only instruments used by the Euclidean geometry in its constructions. And although it would

not be out of place to suppose that the Greeks themselves had suspected that the means used were insufficient for the solution of the three problems which remained for them insoluble, yet they lacked the power to make themselves sure of the matter by means of analysis.

The matter has been put in a new light for us since Descartes founded analytic geometry. Then appeared the true sense of the question upon which so many efforts had been spent in vain:

"Operating upon the diameter of a circle, *by means of the ruler and compass,* can one construct the side of a square having the same area as a circle, or (which comes to the same thing) can one construct a segment equal to the length of the circumference?"

Thus the word "construct" assumed a meaning determined with reference to certain instruments (ruler and compass) which were exclusively to be used; and thus the proposed problem appeared in a new aspect.

If the length of the circumference is to be constructed in the manner pointed out, the number π, which expresses its relation to the diameter, must possess certain well-determined analytic properties.

Hence it becomes a precise question to learn whether such properties belong to it. And in this form we see *a priori* how the problem admits of an affirmative or a negative answer.

The question was resolved, in 1882, by the work of Lindemann, who happily succeeded in extending to a larger field of numbers the methods wisely thought out by Hermite in his study of the number *e*, the base of the Napierean logarithms.

The *answer* is *negative.* We should not then seek to square the circle by means of Euclidean constructions, since the solution of the problem is in that sense impossible.

But as we have already pointed out, that impossibility exists only in relation to the instruments specified.

Since the solution exists, it cannot be in the absolute sense impossible to obtain it. What is required is, then, to think out a suitable instrument which shall be capable of furnishing the solution by meeting all the practical needs.

From considering the question in this new aspect we may say that a satisfactory solution of the problem has been given by the *integraph* of Abdank Abakanowicz, commonly used for the computation of areas.

6. PERPETUAL MOTION.

An impossibility even more absolute than squaring the circle seems to frustrate the desires of those who for centuries have been seeking perpetual motion. But the scientific problem connected with this research, far from proving to be insoluble, has led to the discovery of a leading principle of nature. If the matter is considered in its broader aspect, the requirement of perpetual motion reveals itself as that of a particular relation between the dynamic elements capable of generating the motion of a machine, and the work done by the machine.

Now such a relation is established by the principle of the conservation of energy, through which precisely perpetual motion, as it is commonly understood, turns out to be impossible.

Meanwhile it was observed that if, independently of the inequality of temperatures, one could succeed in transforming heat into mechanical work, a new form of perpetual motion might be reached. We should then have a machine which would utilize, without being obliged to stop, the work done by a body constantly cooling off below the temperature of the environment.

But to the new demand, suggested by the search for such a perpetual motion of the *second species,* the second principle of thermo-dynamics answers, in the field of physical experiments, by denying the possibility of such a transformation. We shall be led to realize the great positive fruitfulness of this principle when we consider that in connection with the first principle of the conservation of energy, the new principle gives to us, by the theorem of Carnot, a way of estimating the work done by heat-machines.

7. ALCHEMY.

We have seen how the problem of perpetual motion ultimately reaches an element which is *quantitatively* invariable in the transformation of energy.

Instead, a *qualitative* irreducibility[1] of matter, in relation to our means of experiment, is the result to which the researches of the alchemists would lead. Thus the problem of "changing base metals into gold" seemed to reappear in a much more general form whence modern chemistry has arisen.

The manifold changes of matter, appearing at first in the guise

[1] The relative character of this irreducibility has been brought to light by the recent researches of Ramsay which seem to prove the transformation of radium into helium and of copper into lithium.

of the miraculous, had struck the fancy of the early investigators, to whom no change in the constitution of bodies could have seemed impossible. But when the criticism of observed facts made it possible to catch a glimpse of "the law" which dominated the varieties of phenomena, the problem of chemistry began to assume its true scientific aspect, rising to the general investigation of the relations and conditions which rule the transformation of matter.

This may be called the new statement of the problem which was hidden in the minds of the alchemists, in so far at least as we take account of the obscure scientific spirit by which their researches were prompted. If, on the other hand, the growth of modern science is considered, one cannot fail to recognize how the very wealth so longed for by the ancient scholars has been surpassed. For the industrial applications of chemistry give, in our times, far greater and more valuable returns than the riches of Midas, which the transformation of metals into gold would have given.

8. THE PROBLEM OF KNOWLEDGE.

No greed for riches has driven philosophers to devote their whole energies to the problems of reality and of knowledge. If they had succeeded in their endeavors, one sole prize might seem to have awaited them; that is, the prize of reconquering, after having passed through philosophic skepticism, that solid and simple faith of men, which is beyond and above all criticism.

But precisely the most logical minds, starting upon this road, seemed to reach the very opposite result. Nothing guaranteed the existence of this asserted reality, which it is not granted us to attain by any means. The idea alone is true, and the ego remains secure as the ruler of a world which crumbles around it.

A marvelous conclusion, to which indeed one might easily make the answer that Diogenes made to Zeno, who claimed to prove that motion does not exist: The cynic, rising from the ground where he sat, silently began to walk.

Just so does positivism answer metaphysical idealism, pointing to the facts which science has collected.

To the pride of spirit, which believes itself sole lord of a world of dreams, and would discover all laws within itself, science replies by showing a reality which spreads about and beyond us, and so escapes the vain claim that it should be subject to our sentiments or to our will.

But really jests are but blunt weapons against philosophers.

And he who contents himself with laughing deserves perhaps to be reminded of the proverb *"risus abundat in ore stultorum."*

In dealing with men of high powers, it surely is wiser to try to understand them, and to draw profit even from their errors. For an absurd conclusion cannot stop the process of thought; and an error in which one sees only the ridiculous, is an opportunity for instruction lost.

How can we doubt that of which every man is most certain even from his earliest infancy?

We cannot understand this save by going back, in our imagination, to that almost forgotten age when dreams are confused with reality, and the image reflected in a glass seems as real as the person standing before it.

Since truth and error enter our minds by the door which the senses open to knowledge, we are soon obliged to be on our guard against illusions.

Man's determination not to be deceived is precisely the origin of the problem of knowledge. The question is always and only this: to learn to know and to grasp reality in the midst of a thousand causes of error which tend to vitiate our observation. It is needful then to establish a relative distinction. By losing sight of such a relativity in order to follow the mirage of a fantastic absolute, metaphysical idealism has brought us directly back to our starting-point, namely the confusing of dreams with real things. Whether we give to the one the name of the other, or *vice versa,* there is no essential difference between the two cases.

It sometimes happens in Alpine excursions that one who does not know just how far off is the goal, thinks he is about to reach it, while he is mounting the rocky summit of some height facing it. But once scaled, a new valley opens unexpectedly to view. One must descend with care. After a fatiguing walk of several hours he finds himself perhaps no higher than when he started. But the time and labor have not been wasted, since even if the summit seems now more distant, in the enlarged horizon, in reality we have come nearer to our goal by surmounting an obstacle which had concealed it. We must not then lose courage, nor give up in a moment of weakness.

Let us then begin our struggle anew, with a right good will! And if the slope is steep, if great crevasses hidden by snow open under our feet, let us climb with care, holding firmly to one another. Let us tie ourselves to the rope and give each other a hand!

This metaphor may apply to metaphysical idealism, which, climbing the rocky hill of a fantastic absolute, finds itself confronted by a deep valley, beyond which glows the reality to be reconquered. We are truly nearer to this real goal, now that we are enlightened as to the relative nature of the question.

Then let all well-disposed men, undaunted by a moment's discomfort, unite to conquer, with renewed efforts, the new difficulty which appears before our eyes!

9. THE DANGERS OF LANGUAGE.

We must, above all, avoid the errors of the past. Therefore we should take notice that language, which we use to express our thoughts, is, in the last analysis, a system of symbolic representations of things. Since language furnishes a process of schematizing, rising by degrees to the expression of more general facts, it allows us to reason about abstract ideas, very far from the immediate reality which appeals to our senses.

But the use of this powerful instrument, which comes to the aid of our mental weakness, is not without its dangers. Taking flight toward the lofty realms of thought, we run the risk of forgetting the meaning of words, which become void of sense as soon as they cease to represent things. Having reached this point, nothing is easier than to use symbols formally, while the development of thought tending toward generality, no longer finds any check in the concrete world to which it remains foreign.

If then you would not lose yourself in a dream devoid of sense, you should not forget the supreme *condition of positivity,* by means of which the critical judgment must affirm or deny, in the last analysis, facts either particular or general.

10. ABSOLUTE AND RELATIVE: THE ABSOLUTE IN THE REALM OF MOTION.

These observations throw a strong light on the classic arguments with which men have attempted to prove the existence of something *absolute,* which must eternally escape our knowledge.

We find in language the word "absolute" opposed to "relative." The word has a significance easy to infer from the use ordinarily made of it, in regard to any subject whatever.

If we are driving in a carriage, we see the trees passing before our eyes, and we say that they are moving with relation to us; but,

in the absolute sense, the trees are staying still, and it is the carriage in which we are traveling that moves. This absolute in its turn becomes a relative thing, if considered from the point of view of astronomy. The trees are carried by the earth, which revolves around the sun.

But the sun itself seems also endowed with its proper motion with respect to the distant stars, whose reciprocal variations appear negligible for a limited period of time.

Nevertheless observations made during centuries have shown that even these stars, improperly called "fixed," are moving, with regard to one another, changing their relative distances to a degree which we must judge to be enormous, if we realize the variation of angles according to which they are seen from a point so distant as our earth.

In conclusion, the motion conceived as absolute in a certain group of facts, appears as relative in a wider field; it is an absolute subject to degrees, corresponding to the need of seeking for our science a more fixed point of support.

I have merely wished to cite an example, without pushing the discussion to the limits that we might reach, in the actual state of our knowledge.

We shall have further occasion to take up the problem: What is the most absolute sense that we can attribute to motion? But it is always a question of giving to the word "absolute" a more extended relative meaning, such as may better satisfy known mechanical relations when taken as a whole.

11. THE ABSOLUTE IN THE REALM OF MORALITY.

Let us choose a second example, in a completely different field of thought. He who *desires* an end *ought* to desire at least some of the means which lead to that end. In this sense the feeling of *ought* appears amongst the determining motives of all continued volition, be it good or bad.

This kind of duty toward oneself is joined with similar duties toward society. These duties, however acquired by the suggestive influence of others, still can be understood only as duties relating to an implicitly accepted end, even if it be accepted through the will of a social group rather than through that of the individual.

Now morality distinguishes between such duties, contrasting the "absolute duties" with the "relative." It admits that the difficulties of accomplishment may excuse the non-fulfilment of the

latter, but on the contrary it decrees that such a reason does not avail to excuse any one from fulfilling the former. Why?

Because the absolute duties in question relate to general ends, the permanent observance of which has for human society a value above any passing sacrifice or hardship however great.

But the value of this end, which has an absolute significance in regard to certain motives of action, appears in its turn as relative when compared with other ends of the same order. The duty which demands its fulfilment, notwithstanding the sacrifice or the injury of the agent, does not equally require the subordination of other duties. The end does not justify the means. And the moral conflict can be reconciled only by a comparative judgment of the ideals in question and of their subordination to a higher ideal.

But there is no ideal which may not be surpassed, and the loftiest end, which within a certain social group, and at a certain epoch has an absolute sense, becomes relative in a broader comparison with the morals of different peoples who have different modes of life.

But this objection may be raised: Is not justice the ideal which absolutely cannot be surpassed? In fact always and for all stages of social evolution, the human idea of justice expresses the highest synthesis of appreciative judgments. But cannot these judgments themselves always be extended to a larger circle of relations? And does it not therefore follow that their general and abstract expression can never be regarded as complete?

The absolute value of morality, then, signifies nothing but a larger relativity. Such a conclusion is imposed upon whoever scientifically conceives morality as a fact, regardless of all possible considerations of advantage or disadvantage which could be connected with this result. But the dreaded danger of evil result does not exist for him who keeps before his eyes the preeminence of general ethical ideals over the motives of individual actions. For this is the only thing of practical importance expressed by such an absolute. Meanwhile the statement that morality is relative, tends in itself to raise the standards of our judgments and conduct, above all in the relations of different peoples, in different conditions of life. Nothing is more unjust than to extend the canons of our morality to men unlike ourselves; and the absurd pretense of imposing our rules upon them in the name of a natural superiority, ought simply to make a philosopher laugh, if the practical consequences did not suggest sadder reflections to his mind.

12. WHAT IS THE ABSOLUTE IN A TRANSCENDENTAL SENSE?

In the above examples, the absolute, in agreement with the common use of language, appears as signifying a deeper relativity, and one more distant from us.

It is true, in such cases, that a vague consciousness tells us the word is not used in its strict sense. But we are not usually engaged in defining such a sense, in the larger extension which is thus dimly implied.

Let us now see what the word has come to mean to the philosophers.

Since there are degrees of relativity, we may claim to reach the end of an infinite series, in order to attain something which shall no longer have any relative aspect, and which may then properly receive the name of absolute.

An infinite series of degrees come to an end? It is evident that the proposition is self-contradictory. But this manifest absurdity does not yield in presence of an illusion that is deeply rooted in the human mind.

There is a peculiar advantage in making use of symbols, in that we may represent the term occupying in a given series any place whatever, without successively completing in our thoughts the operations needful to reach that place. Thus, for example, we can reason about the number 164, 792, 843 without counting one after another all the units of which it is composed. Or in the same way, we may undertake arithmetical calculations concerning 2^{1000}, without following out the thousand successive multiplications indicated by the symbol.

In these cases the mind accomplishes, so to speak, in a shorter way, by virtue of logically established relations, a series of possible operations, which would merely require more time. And the thought pauses upon a well-defined object in the series itself.

But the habit which is thus contracted of substituting a symbol for the operations which constitute the real definition, gives rise to the illusion that the thing may be defined by the symbol; and therefore that it should suffice to denote by a word the *last term* of an infinite series, in order that an object should correspond to this word. Nevertheless the symbol is in this case devoid of sense, since the *transcendental* operation is impossible which we attempt to indicate by means of it. Abbreviated procedures suffice for the notion of performing more rapidly a finite number of opera-

tions, never an infinite number. Infinity cannot be exhausted in however long a time, or however much we toil at abbreviating or grouping together its terms.

13. TRANSCENDENTAL PROCEDURES CONSIDERED IN THE LIGHT OF PHYSIOLOGICAL PSYCHOLOGY.

We can state this last proposition in a more exact form, which may put in clearer light the illegitimacy of the transcendental procedures mentioned above.

Physiological psychology in fact allows us to *measure* the time required by an act of thought. And since in every case there is a minimum of duration, we could not possibly grant to the human mind the faculty of accomplishing infinite acts, in any determinate time whatsoever.

14. TRANSCENDENTAL PROCEDURES IN RELATION TO INFINITESIMAL ANALYSIS.

We need not then be surprised that the opposite supposition, used as the basis of a vicious mode of definition, leads to numberless absurdities

Infinitesimal analysis forms a field in which such absurdities were most plainly manifest before these transcendental processes of reasoning were happily banished from that field. The critique of the concepts of *infinite* and of *infinitesimal,* of *series* and of *limit,* seems to form the most fitting preparation for thoroughly comprehending the sense of the foregoing observations.

The first thing pointed out by such a critique is that the word "infinite" cannot be applied to any given number or quantity, but denotes only a mode of increase of a variable quantity, which may be susceptible of receiving a higher value than any preassigned constant value. This is exactly expressed by saying that the infinite has no *actual* sense according to the acceptation of *Leibniz,*[1] but only a *potential* or *genetic* sense.

The same holds good for the infinitesimals. The importance of this way of conceiving things consists in recognizing the absurdity of expecting to *define a number,* by means of an *infinite series,* as the *last term* of that series. It may be that outside of

[1] It is true that this statement ought to be modified with regard to the *systems of non-Archimedean numbers,* lately constructed in different ways, by Veronese, Levi-Civita, Hilbert etc. We may however leave these constructions aside. It is sufficient to say that they do not imply the use of transcendental processes of definition.

the series there is really to be found a number which constitutes its *limit,* which the terms of the aforesaid series continue *to approach*; but the existence of this limit only expresses in substance a property of the *mode of variation* of the terms of another series, built up from the differences between the limit-number and the terms of the original series. Therefore *the existence of the limit cannot be proved by the mere fact of the existence of the series.* In other words, *the limit cannot be defined exclusively by means of that series, but only by comparing the series to something which is independently given from outside.*

The practical value of this statement is well known by this time to all those who understand infinitesimal analysis, since infinite algorithms generally give rise to *series having no limits,* and reasoning as if these expressions were capable of defining such a limit leads to the most singular absurdities.

For example the series obtained by an infinite process of summation can appear not only as *converging* towards a limit but as *divergent* or *indeterminate.* To these two last categories belong respectively the series

$$1+\tfrac{1}{2}+\tfrac{1}{3}+\tfrac{1}{4}+\ldots,$$
$$1-1+1-1+\ldots.$$

But the use of the series in our calculations would permit us to prove the equality of any two numbers whatsoever!

This suffices; for even in the handling of convergent series, we must not forget that they are only conventional means of representing a number that is their limit and that does not belong to them. If we were to forget this fact, we should indeed be led to admit a transcendental element, and to regard the series as *the sum of an infinite number of terms.* In the latter case we should feel authorized to work upon these according to the properties of a sum, interchanging, for example, the order of the terms. But in this manner, for example, from the convergent series

$$1-\tfrac{1}{2}+\tfrac{1}{3}-\tfrac{1}{4}+\tfrac{1}{5}\ldots,$$

one may form at will series converging towards different limits, and also diverging or indeterminate series.

"To eliminate all transcendental processes of definition or of reasoning": that is the essential condition for understanding the infinitesimal calculus—a condition which Cesaro states at the beginning of his admirable lectures, thus warning the reader to banish from his mind all metaphysical ideas!

To the teachings which have been afforded us by the infinitesimal analysis, let us add some which spring from the modern *theory of assemblages.*

Here the process of transcendental definition has presented itself in the very construction of certain "assemblages" taken as the "totality of the infinite multitude of entities which have certain specified characteristics."

Among the examples brought to light by the studies of Cantor, Du Bois Reymond, etc. let us choose one very simple one to which Russell has lately called the attention of geometers.

We may construct in several ways an assemblage

$$s \equiv [a],$$

composed of whatever entities you will $a_1, a_2, a_3 \ldots$, —but subject to the rule that s is not itself contained among its own elements—an assemblage (such then that no one of the entities a coincides with s.)

Now let us define Σ as the *assemblage of all the assemblages s to which the given property belongs.*

$$\Sigma \equiv [s].$$

The first condition is that Σ must not be found among the elements s otherwise there would be a contradiction in the property supposed by the s. But if, on the other hand, Σ is found outside of the assemblage $[s]$, this assemblage does not exhaust all possible s which possess the aforesaid properties.

The contradiction shows that the concept of Σ is illusory, and thus makes plain the vicious nature of the transcendental process by which Σ was defined.

15. THE PSYCHOLOGICAL VALUE OF THE ABSOLUTE.

But the logical analysis which reveals the vice of the transcendental definition, does not exhaust the question of the absolute. How otherwise could we explain the place held by the absolute among the beliefs that are bound up with the deepest sentiments of the human mind? In what way could an error in the statement of a problem give *value* to a symbol which we have seen to be devoid of sense?

In order to answer such questions we had better go back to the considerations concerning "the Absolute in Morals." We have already noticed the peculiar character of the human will, in that it is

eminently progressive, that it arranges, namely, the ends it proposes in a hierarchy, so that the nearer end is subordinated to the more distant.

The stability of such a hierarchy demands that at every moment the higher aim should act upon the will as a sufficiently strong motive against the motives of the moment which might tend to turn it aside or to weaken its determination. And this auto-suggestive force is given by the trials which the will itself has made of its own firmness, and is shown in the resulting consciousness which the will has of *not changing*.

One sees then how the progressive extension of this hierarchy of ends and its stability constitute two contradictory psychological requirements for the human will. A new higher end cannot come to modify the ends already accepted by the will, without diminishing its faith in its own consistency and force.

When, in the life of the individual or of society, the outlook over a field of purposes enlarges rapidly, there follows one of those *critical periods* which are characterized by the disorganization of the will. This state of impotence, annulling the human personality for the moment, usually contains within itself its own corrective, because it destroys the interest in the search for new aims. Progress being thus stopped, all the discordant motives striving within the troubled mind finally engage in one supreme battle; and when a determination is declared victorious, by seeming able to subordinate the others, the mind leans upon that with all the energy of the reaction which the need of escaping from a sad condition gives.

Any man passing from childhood to youth, or from youth to manhood, usually passes through such a critical period as that just described, and comes forth from it by his own courage, or by the help of others.

Just so in certain historical moments, analogous crises of the will are produced in the social order. We have then periods of revolutionary disorganization which follow an over-rapid progress and bring it to a close by exalting unduly the concept of that authority which had established itself generally.

The psychological value of the absolute is inherent in the conditions of progress above described. And it would be easy to furnish historical proofs. The illusion of transcending the infinite series of ends answers to the need of seeking *beyond* an end of agonizing doubts, by ending with imperative authority the conflicts of the will.

In this sense the absolute is a condition which sets the spirit free, rather than a guiding idea, and through it all the faculties are concentrated and drawn toward one single point, while all extraneous impulses and all criticism are inhibited by the consciousness: *credo quia absurdum est.*

Such a state of mind, essentially emotional and religious, constitutes a problem for the psychologist, but the philosopher who is dealing with the theory of knowledge has no motive for concerning himself with it.

To explain how and why, together with the weakening of religious faith, in modern times, the absolute has become the object of a research which tries to construct science out of it, would be indeed instructive, and not alone from the historical point of view. But such an explanation would need a much longer examination.

It must suffice us to show, by means of the preceding considerations that the absolute, taken as an object of rational construction, is no longer absolute, and, through the very criticism itself, loses the value of supreme guide, dominating all the aims of the will, and all the motives of intellect and of sense.

What then remains of this pretended metaphysic but a mere document of the human mind, a mind at once feeble and domineering?

Icarus, poised for a flight through the heavens, fell headlong into the depths of the sea. Reason, discouraged, sinks into the depths of the unknowable.

16. SUBSTANCE AND APPEARANCE.

We have seen how the wide-spread sophism which says that "the relative presupposes the absolute," rests upon a verbal illusion, that conceals a process of definition wholly void of sense.

All the similar antinomies, of which classical philosophy is full, may be explained in an analogous way. They may be put into the form of an infinite process, or they may appear as a simple negation; but in the latter case the negation is purely formal, and serves in the same way to construct for us a statement void of meaning.

The antinomies of this sort, being true *up to a certain point,* if we do not give a strict sense to the terms, gain just from this fact all their force.

We speak, for example, of the *substance* of things in contradistinction to their *appearance.* Carbon and diamond are of the same substance though quite different in appearance. On the con-

trary, under the similar appearance of a diamond and of rock crystal, are hidden quite different substances.

In such cases an important distinction is established between the immediate sensations which we refer to any object whatever, and the totality of its relations to the surrounding world, of which we can partially acquire a mediate knowledge.

And this observation goes back to Aristotle, for the philosopher remarked, for example, how the oar, plunged into water, looks as if broken, when it is really whole. But the original sense of that distinction underwent a change during the Middle Ages, when the idea of the absolute was superimposed upon the Hellenic civilization.

Let us see what Kant has made of this distinction! Appearance, or as Kant expresses it, *the phenomenon,* is conceived as contrasted with the essence, or *noumenon,* taking this last term in an absolute sense.[1]

Kant abstracts, in considering any body, from all its relations, which are perceptible to us, either in an immediate or in a mediate way; what remains is the true essence of the body.

Such a non-sense would be truly inexplicable, were it not joined with an anthropomorphic representation of the world. Here it is fancied that, if we could enter into a stone, we should experience sensations adapted to reveal to us its real essence.

The confused state of mind which corresponds to the absolute mode of considering essence, recalls precisely the anthropomorphism through which we must probably have passed during our earliest infancy.

But really it would not be strange if we should arrive at agnostic conclusions as to this so-called essence,[2] so defined that no meaning at all remains in the word!

17. THE UNKNOWN.

We do not wish thus to deny the feeling of a "great mystery of the universe," which reflections upon the idea of substance excite in our minds. Since we conceive that manifold relations bind all things together, we are led to discern behind these an unknown to be unveiled, and to represent to ourselves the impossibility of exhausting

[1] We here allude to the *noumenon* understood in a *negative* sense, as in Kant's discussion in section III of his "Transcendental Analytik."

[2] The analysis of Kant leads precisely to the unknowability of the *noumenon,* which he expresses by affirming the impossibility of understanding a *noumenon* in a *positive* sense.

the objects that belong to this realm. But the affirmation of the unknowable does not adequately express this conception!

In agreement with our own Ardigò in his criticism of this point in the doctrine of Spencer, we cannot admit that to a reality accessible to our knowledge, there is opposed a mysterious reality, which *necessarily* must escape all efforts of our thought. On the contrary the foregoing observations show a series of objects equally accessible to research; but as that series appears to us to be without limit, we see that our desire for knowledge can never be completely satisfied. Truly this is a fortunate thing for humanity, before whom an endless progress is thus opened!

18. THE DISTINCTION BETWEEN SUBJECTIVE AND OBJECTIVE ACCORDING TO KANT.

Akin to the distinction between appearance and substance is that between subject and object, or between subjective and objective.

We may now dispense with repeating that such a distinction, taken absolutely, is devoid of sense, in that it corresponds on the one hand with a transcendental view of the thing in itself (which is confounded with the substance), on the other hand again, with a transcendental view of the ego, considered as a substratum independent of the various individual attributes which are added to it in one person. It is always the same spirit of founding one's criticism on the basis of absolute distinctions, which furnishes the foundation for Kant's agnosticism.

But only he who considers things from a special point of view can interpret Kant's philosophy as leading towards a skeptical result that should agree with the postulates of the positive philosophy. This side of Kant's doctrine seems rather to be intended to reconcile certain practical requirements, in so far as "it allows precisely practical data to fill, in the extension of knowledge, the place which speculative reason is seen to be incapable of filling."[1] Still we must admit that, by bringing to light some of his defects, we have not lessened the value of the revolution of the older metaphysics accomplished by Kant. Broadly interpreting the spirit of Kant, we perceive in fact that the distinction between subjective and objective does not remain a sterile antinomy in the thought of the philosopher, but becomes for him the starting-point for a new conception of scientific reality, a conception which positivism has reached by another road.

[1] Cf. *Critique of Pure Reason*, 2d ed., Preface.

We believe rather that to the more restricted view of positivism the philosophy of Kant may even add something scientific if we agree to keep only the spirit that appears in its best possessions, and if we advance from other foundations to a new construction.

The teaching that "in consciousness a personal element (subjective) and a real element (objective) differentiate themselves from one another, and that the latter, varying in different persons, is joined with certain general forms of human sensibility and intelligence"—this teaching is in fact full of positive significance, if we properly estimate the distinction, excluding all pretense of finding in it anything absolute.

But we must therefore not only set aside the extremes of post-Kantian speculation, but likewise the very judgments of the master about the "*a priori* anticipation of forms of possible experience," (compare *op. cit.*) and about the "objective reality which is given to our knowledge *a priori* by the possibility of experience" (cf. *op. cit.*). For these judgments are in themselves ambiguous, and in their legitimate interpretation and application, retain traces of their original vice of understanding transcendentally the distinction between subjective and objective. And by their judgment in their recognizing implicitly a certain objectivity of the structural laws of mind in the constructions of geometry or mechanics,—the door is after all reopened to that metaphysics which Kant wished to have condemned forever.

Indeed apart from the development of philosophy in the past century, what was most vital in that critical philosophy has left the clearest traces in the realm of science, where it has had a most awakening effect. And upon exactly this basis the new criticism should arise, and throw its light upon the positive problems of knowledge!

19. THE DISTINCTION BETWEEN SUBJECTIVE AND OBJECTIVE CONSIDERED POSITIVELY.

Has the distinction between subjective and objective a positive content with regard to our knowledge? In what manner will it assert itself since we have seen the antinomy between subject and object arise, when taken transcendentally? Let us consider certain examples which seem adapted to lead us by induction to a suitable definition.

Let us take a little box of cube-shaped blocks, such as in Froebel's system are given to children for toys. The bottom of the box

is divided by two black lines into four squares, equal to the faces of our blocks. The child thus learns that "to cover the bottom of the box one needs *four* blocks."

We all agree in granting that this assertion contains an *objective element.* If the box were larger in comparison with the blocks, *six* or *eight* of them might be needed. Nevertheless some *subjective element* enters into the expression of this very knowledge. In the first place, there is the *verbal form* of the word "four," which is differently spoken by different people, and for which a Frenchman substitutes the sound "quatre," a German "vier" etc. In the second place there is the *psychological form* of the idea in question.[1] One child imagines the "four" by ideally associating the blocks with the fingers of his hand with the thumb turned in. Another child associates them with four of the balls on his counting-frame, and a third child, who cannot yet count, has taught himself to shape with stones upon the blocks the arcs of a circle, each being a quadrant. Thus he succeeds in laying apart as many blocks as are needed to cover the bottom of the box, by placing them in turn, one beside the other, in such a way that the arcs form approximately a circle. The last child, like the others, has the objective knowledge in question, but he has gained it, not through the representation of number, but rather by means of the geometrical figure.

We may say that these children have *objectively* the same knowledge, since they *agree in the prevision* required of them by providing equally well the blocks which will cover the bottom of the box, while this prevision would *not* be found to be verified in the case of *another box,* or of *blocks of different sizes.* We may say that the knowledge of the children is *subjectively* diverse, because they obtain their prevision in different ways, by means of different images.

But if we carry our criticism a little further we perceive:

I. That such a prevision is not possible without some mental image, and therefore that a *purely* objective consciousness is impossible.

2. That the subjective manner of representation has an influence over the prevision itself, and over other analogous previsions, so that we must admit that it contains some objectivity.

This comes out very clearly in the example just used. The

[1] A psychological difference is sometimes expressed by differences in the words themselves: as for example, the number *ottanta* (80) is represented by an Italian as 10×8, by a Frenchman as 20×4 (*quatre-vingts*).

child who pictures to himself geometrically the fact we have stated, *knows less* than the others, in that he does not quickly see that his four marked blocks will cover the bottom of the box, even if they are distributed so as to form, with their four arcs, a figure differing from the circle (v. Fig. 1). From another point of view the above geometrical representation *teaches more,* that is, to recognize that the bottom of the box can be covered by arranging the blocks in a given way and in a given order etc.

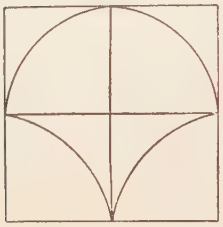

Fig. 1.

From this example we can already see that *the subjective element and the objective element are not two irreducible terms of knowledge, but they are rather two aspects of this knowledge resulting from its being compared with other knowledge in one same person or in different persons, in relation to one thing or to different things.*

We find the objective element wherever there is an agreement of *previsions,* in however diverse modes these may be obtained by one or more persons. We find the *subjective* element in the plurality of these possible *modes.* But to whatever degree these two elements become *more and more distinct* with the growth of knowledge, yet *absolutely distinct* they will never be. For the concepts of objective and of subjective arise all the time through abstraction, from a comparison of cognitions; which cognitions will be always capable of extension.

Consequently some objective element will always be contained in the subjective aspect of knowledge, and so also will there be some subjective element in its objective aspect.

In any case the process of distinction may be followed much further than appears from the example just cited. Always using for our basis the *comparison of cognitions,* we may in fact rise above that objectivity, which appears whenever we compare different people's knowledge of one same thing. And if in a similar way we compare the kinds of knowledge relating to different objects, we may succeed in recognizing the subjectivity of representations which are yet common to all men.

This is a new *scientific stage* of the distinction between subjective and objective, in which a new condition reveals itself, namely, that "the mode of representation leading in a given case to a verified prevision, leads us into *error* with regard to other possible pre

visions." This may be expressed by saying that to the subjective aspect of knowledge are joined the *appearances* of things which do not correspond with their *reality*. The distinction thus obtained then leads us to *correct* our knowledge as primarily gained by dividing it into two parts, one of which (the objective element) corresponds better to the larger *complex of previsions*, while the other (the subjective element) added to the former offers a prevision, relative to the single incongruous case or *explains*, as we say, the *fallacious appearance*.

20. SUBJECTIVE AND OBJECTIVE IN THE PROCESS OF MEASUREMENT.

We may learn something as to this point by the analysis of a simple and instructive example.

When we speak of the *size* or of the *dimensions of an object*, we affirm a complex cognition which includes the agreement of manifold previsions, in relation to possible experiences of touch, of sight etc.

Now let us suppose *as known* the dimensions of an object placed before our eyes. Sight furnishes us a certain immediate knowledge of these dimensions, in which are mingled, nevertheless, various elements which we ought to subject to an analysis.

First we must take account of the distance and of the position of the object, and correct the causes of error which arise from this element of relation between the subject and the object, thus changing in several ways the conditions of our observation.

But even then there still remain, in our consciousness, some elements depending upon the structure of the visual organ, which, as physiological psychology teaches us, varies in different men, though in any case it obeys certain general laws of sensation. Thus, for example, two points of the object, which are separated by empty space, appear nearer than two other equidistant points which are joined by a tract of continuous matter. And in like manner equal lengths are differently judged according to their uniform or varied coloring etc.

We have seen already, in the example cited, the influence of the subjective element which is inherent in sight, and how the sensations of touch enable us to correct that influence. But even without re sorting to the more exact processes of measurement, we can easily understand by means of touch how experiences may lead us to

recognize directly the organic causes of error, without leaving the domain of visual observation. It may suffice simply to notice the coloring of different parts of the object, to substitute empty space for such and such filled parts etc. And when once rules which may be applied to aerial perspective, taken in a broad sense, have been established we have the means of correcting our sight impressions, and hence of reaching a more adequate knowledge of the dimensions submitted to our judgment, so as to obtain previsions more in agreement with the various possible experiences of sight, in one person or in different persons.

If now we drop all restrictions as to the use of one sense rather than another we shall find, without leaving the field of measurement, material for further interesting reflections.

The determination of a measured length, constituting a comparative judgment between an unknown length and another taken as the unit of measure, is effected by means of the senses, aided by suitable instruments. The instrument used secures a certain degree of precision, which may be regarded as an objective datum for the judgment. But beyond this we should take into consideration the *manner* in which the said instrument is used, the physical and psychological conditions of the experimenter, and a thousand other accessory causes, by which the results of experiments, requiring a certain degree of accuracy, vary from one time to another for the same person, and at all times together for different persons.

Now, leaving out of account the *systematic errors,* and concerning ourselves only with the *accidental errors,* we meet with the singular and instructive fact that the average of the results obtained by the same experimenter tends to differ from the *actual measure* by a regular error, which goes by the name of *personal equation.*

This fact may be perceived in two ways, either by using more perfect instruments, or by comparing the observations of different persons. But the personal equation once established is sufficient in itself to correct systematically the determination of measures, and we reach a greater degree of precision, which shows itself in a more exact agreement between the results of the same experimenter and of different experimenters.

The observations recorded above throw light upon the positive content of the statement relating to the subjective element in consciousness.

In fact, through the preceding example we see once more that

there can be no question of giving to the relations of the subject and the object a strictly transcendental meaning. The distinction certainly loses none of its importance through its relativity. The existence of a datum due to the nature of the sense organs is clearly established, however true it is that the very knowledge itself which we accept as exact is mediately derived from the same senses whose errors we are bringing to light.

But there is another aspect of the above considerations which it is important to bring out. In view of the law of error above referred to, one substitutes for the totality of many and varied observations, their arithmetical mean, which is supposed to be nearer to the exact measurement.

Two questions arise. First of all we see that the idea of an *exact* measure forms part of a mental process which leads us to correct the error of our sense impressions. And the followers of Kant lay especial stress upon this essential point.

What is an exact measure? Is it not the absolute in the world of measures? And are we not thus forced to grant a place to that same absolute, which we have already declared to be senseless? Let us examine the question dispassionately.

If we were asked: "Can one conceive an *ultimate* term in the constantly approximating determination of a measure?" we should answer "No." In this realistic sense, the exact measure has no meaning. The hypothesis of a strictly immediate determination of a measure meets with various kinds of objections. What we know or suppose about the constitution of matter, and what we admit in regard to light (above all the idea of a *wave-length*), throws obstacles in the way of such an hypothesis. So that it is not difficult, for example, to assign to the least length visible with a microscope a theoretical limit, not very far from the practical limit actually reached.

But all this makes little difference to us if we are using the *hypothesis* of an exact measure as the starting-point of a reasoning process.

The worth of the hypothesis consists only in admitting that the facts inherent in the experimental determination of measure can be represented by a logically well defined concept (the concept of a number), in such a way that the reasoning processes founded upon such a concept will lead to previsions verified by experience. Now in this subordination of the data of sensation to such *concepts,* there is actually an *element of psychological nature,* the importance of

which for the theory of knowledge we shall have occasion to notice later.

A few words further as to the *postulate of the mean*, which is less directly bound up with our subject.

Lipmann cleverly said to Poincaré that in such questions, relating to probability, the agreement of scientific men is unanimous, since mathematicians consider them to be decided by physical experiment, and the physicists believe that the mathematicians have settled the matter. Whereupon Poincaré, reporting this remark, justly observes that the mathematicians are right in their view. For the principle in question cannot be established by mathematics. It is an empirical rule.

The choice of the arithmetical mean to represent a series of observations, corresponds to the choice of that number for which the sum of the squares of the differences between the results of several partial observations is smallest. This choice is *a priori* arbitrary. But the principle upon which it depends is justified by the fuller agreement obtained by comparing various series of observations. In sum, the physical notion of measure corresponds to an interval, which we try to make as small as we can, within which are enclosed the numbers furnished by the process of measurement. This interval is diminished if for the numbers obtained by single observations we substitute the averages of similar observations. Such is the value of the postulate of the *mean* upon which Gauss has built up his *theory of errors*. One takes the arithmetical mean, reducing to its minimum the sum of the squares of the differences pointed out before; precisely because one seeks, as a result, to reduce the interval which separates, in either direction, the extreme numbers obtained, from the number equidistant from them.

21. SUBJECTIVE AND OBJECTIVE IN SCIENTIFIC CONSTRUCTION.

In consequence of the preceding observations we shall be led to discuss the problem which deals with *the positive definition of reality*. We shall consider this problem more fully in Chap. II, by undertaking a critique of facts and theories.

Let us here merely notice how very instructive the example of measure is as to the light it throws upon the scientific value of the distinction between subjective and objective in knowledge. For this distinction, if we lay aside any pretense of understanding it transcendentally, becomes the starting point of a *method of pro-*

gressive correction for the extension of science. If such a view cannot satisfy those who, in whatsoever field, like to think of a task of scientific research as finished, it answers on the other hand for the needs of progress, and thus acquires a far more positive and satisfying significance for practical purposes.

Undoubtedly science aims at an increasingly objective knowledge. Hence throughout its development, it leaves out of its dogmatic expositions those elements of knowledge thus far acquired which seem to be subjective. But the elimination of the subjective must in the future be pushed still farther in a more advanced investigation in which the correction of its attendant errors shall have proceeded farther. And on the other hand those subjective elements, rejected as a sort of residue of our elimination, will also yield something objective, when sifted by a new criticism.

Thus the constructive progress of science may be compared to the motion of a swing, which the person swinging in it tries to push forward as high as he can. For every push forward there is also a more pronounced backward motion, and this very fact increases the impetus.

Science, considered as to its growth, not only rises to ever greater objectivity, but by contrast carries to a higher point the subjectivity of its representations which are its means of conquest.

22*a*. A CRITIQUE OF POSITIVISM.

The constructive scheme thus pointed out tends to integrate that view of science as an ordered whole which belongs to the positive philosophy.

The movement of thought known by this name owes its origin to a reaction against the arbitrary conceptions of metaphysical idealism,—conceptions so much the more dangerous in that they pretended to rise to great, nay, absolute objectivity, just where they only reflected the exaggeration of subjectivity.

While Immanuel Kant, in his *Critique* denounced the fallacy of that so-called metaphysical objectivity (yet opening the way for a sort of unjustified encroachment of the subjective upon the objective), Auguste Comte undertook a negative critique of all metaphysical objectivity. To the manifold and discordant philosophical systems, he contrasted the ordered unity of science, and brought to light its characteristic as a knowledge of "facts" belonging to science.

Icilio Vanni acutely pointed out that this character of *real*

knowledge is found by Comte in the *agreement of men*, that is in the *social* value of science, as contrasted with the *individual* value of metaphysics. We have just pointed out, beside this important aspect of the distinction between the objective and the subjective, some other features which belong to the sphere of the individual.

However, the *positive spirit* which inspires all Comte's work, and from which it receives its name, shows itself by a rigorous selection of those cognitions to which the character of objectivity is ascribed. Quite apart from the school of men of letters and sociologists, who have specially attempted to develop certain results of Comte's latest constructions, this positive spirit has exerted a strong influence upon various branches of science, and has reached its highest expression in the field of physico-mathematical knowledge from which the master obtains the characteristics of his thought.

Meanwhile that influence in all regions of knowledge is shown in two ways: by an effort to reach some form of knowledge, which shall be more and more independent of different subjective interpretations; and by a complete disinterestedness towards all that concerns those interpretations, and by a summary condemnation of the metaphysics which they nourish.

We have already said that this negative side of positivism, which somehow seems to gain the most favor, is in our eyes its weakest side. The resulting critique of metaphysics, on the one hand, grants to the latter more than it deserves, while on the other, it includes in one dogmatic condemnation something in that system of metaphysics which really deserves to be vindicated.

22b. POSITIVISM AND METAPHYSICS.

Is there something worth vindicating in the old metaphysics? And is not this very doubt enough to cause all scientific men to ostracize whoever proposes such a thing?

We would ask that before judgment is pronounced the accused should be heard without the presumption of his guilt.

We first need to know: "What is commonly understood by metaphysics?" Few, perhaps none, among the positivists have asked themselves that question. Or at least they have often paused at the answer: "*Metaphysics is the science of the absolute*, and is placed above physical relativity;[1] this *absolute is unknowable*, and hence the so-called science which deals with it is empty."

[1] To such honor has come the word by which was originally understood that book of Aristotle which in his works came after the Physics!

For us, on the contrary, *the absolute* is an unmeaning symbol, defined by a vicious method, and so, from this point of view we should grant *too much* to metaphysics if we accorded it the *existence* of an *object,* even if unattainable, to which it may refer.

In any case our definition of metaphysics is incomplete. Metaphysics not only puts together symbols without sense, which claim to have a transcendental meaning, but it labors to represent its object by the means of *images,* which have a concrete meaning.

Indeed if we attempt to examine even slightly one of those *ontological systems,* which contain the most genuine expression of the metaphysical spirit, we soon see that the *entities* by the aid of which, according to such systems, the world is made, represent nothing but *images of real things.* The authors of those systems may indeed warn you that the substances, ethers or fluids which they conceive are quite different from the concrete objects commonly meant by those words, inasmuch as they represent something beyond phenomena. Still you will soon discover that that difference consists only in regarding as united properties belonging to different things, by means of a mental process of association and abstraction.

In the last analysis, an *ontology is a subjective representation of reality, a model fashioned by the human mind, whose elements, taken from real objects, are so combined as to take account of a certain order of knowledge, from a certain point of view which is arbitrarily treated as universal.*

The earliest ontological systems recorded by history show roughly this character. As for example, the system of Thales of Miletus who explained the moisture of the seed and of the plant, the origin of earthquakes and the nature of the geographical environment of Greece by considering "water" as the first principle of all things. In more highly developed constructions the processes of association and abstraction are more complex.

But in modern philosophy the transcendental method of thinking of the *universality* of the system is added, a method which claims the ability to explain not only all possible new data of reality, but also reality itself taken *in its actual infinity,* and hence the absolute as opposed to the relative.

The theological origin of contemporary metaphysics sufficiently explains the exalted character attributed to its doctrines. In any case we can in some sense see that degeneration by which there is introduced into the ontology a faultiness penetrating the whole system, should be the natural development, in accordance with psycho-

logical law, of the fundamental principle of this knd of philosophy, which, after a few preliminary observations, isolates itself completely from the external world. Indeed such isolation allows the thinker to imagine that his own conception of reality is perfect, in such wise that the *infinity* of the universe, which is a characteristic of the *genetic* process of gaining knowledge, this infinity, I say, cannot be represented in such conceptions save by granting to the *infinite* an *actual* value.

But, without delaying further over the criticism of those transcendental procedures, the vanity of which we have already denounced, let us recognize rather that even in the ontologies of modern metaphysics, there is always a *system of images,* a *model,* which may be well adapted, sometimes to some order of real facts, and which may, in any case, prove useful in the development of science, by promoting new associations. And solely to this element it is due that even in the strangest systems of metaphysics, we meet something which seems to prelude a scientific discovery or view which in reality is obtained later.

On the other hand, side by side with the building up of metaphysical ontologies which boldly aspire to furnish a definitive and complete science, never inquiring if such adjectives applied to the name science have a meaning, other ontological systems have been constructed, and are being constructed every day, enclosed within the limits of some one type of knowledge. Nor does the human mind lack a tendency to isolate itself in the contemplation of these systems, and to extend their validity beyond their original field.[1]

These decidedly modest and useful constructions, which are often confused with science, have not escaped the implacable criticism of Comte, who denounces them as metaphysical. The ether or the fluids, with which the physicists hypothetically fill the invisible world, have found no favor with him; and the same may be said of many other similar conceptions, even now received by many

[1] A characteristic example is the *doctrine of evolution,* which has become as shaped by Herbert Spencer a true *system of metaphysics,* although it is not vitiated by transcendentalism, for which the author has left room only *outside* of the positive part of his philosophy.

In order to extend and to justify the doctrine of evolution beyond the field of biology (for which it is a scientific theory) the illustrious philosopher has been led to set forth, in his *First Principles,* general propositions such as "the *instability of the homogeneous,*" its differentiation of self under the *"incident force"* and others of similar kind: propositions to which no student of mechanics will assent without careful specification of the subsumed; but in which every one will be able to recognize the value of images which are quite happy in certain relations.

scientific men. Whoever has a positive type of mind will be disposed to grant that the condemnation of Comte is justified in so far as such systems and theories claim to be understood as objective knowledge. But that those theories—we mean also the metaphysical ones—have no value of their own as psychological images in the growth of science, that is a thesis which cannot be received without a more profound critical study of that growth itself.

But it is now time to carry our critique of positivism into special scientific fields.

23. PHYSICAL POSITIVISM.

In no other branch of the knowable has the positive spirit received so full an expression as in some views recently brought forward in the realm of physical science. This will seem quite natural moreover to any one who considers that precisely to physics is due the first realistic conception of fact, and in physics originated the experimental method.

The purest and highest type of positivism is found, for example, in the works of Mach and of Kirchhoff. These men are so progressive in their positivism that they have succeeded in banishing from the theory of mechanics the notion of *force*, as something which seemed to them to correspond to a subjective element in the view of motion, notably in the case of astronomy. Physical theories, in so far as they lead, in each group of phenomena, to a differential equation which is supposed to contain only facts, are systematically despoiled of everything in them which has the value of intuition.

From such a conception of things, there follows an unexpected consequence, brought to light by a significant observation of Poincaré. If a *mechanical explanation* of a certain class of phenomena is possible, an endless number of other explanations of the same sort are also possible.

We have, for example, a mechanical explanation of light, according to which it is attributed to a certain mode of vibration of an *ether*. This alone makes it possible to imagine, in endlessly diverse ways, diverse series of vibrations which agree equally with all the phenomena of light.

This statement looks like a paradox. Mechanical theories, differing one from another, can be *true* at the same time, that is, they may equally correspond with reality, in so far as they contain the same facts, and differ in the degree of subjectivity which their representation may possess.

What a progress from the point of view of logic. After so many idle discussions have been settled in an unforeseen way, that we have been led toward a choice between *equivalent* theories, the end of it all is that we lay aside every particular view of facts, in order to ask, in each single case whether they fit in with a mechanical explanation. Thus the spirit of physics is emancipated from certain traditional procedures and it seems difficult to carry positive thought further in this science.

But this emancipation is not complete, since the human intellect still needs to shape for itself mechanical, concrete models, and we should be wrong to consider two theories perfectly equivalent, the subjective values of which are different.

It is rather from the psychological side of these theories that the force comes which helps them on the road to further discoveries. And so, in every field, a small number of theories, corresponding to the need of simplicity which is adapted to our understanding, have been conceived and seriously discussed.

The English spirit of a Maxwell or of a Thomson does not fear to follow theories of this nature into their most minute particulars; and the concrete view of the motion of the ether, suggests to them the discovery of facts which challenge the admiration of the whole scientific world.

We still remain in the domain of physical knowledge if we briefly examine some questions relating to the *atomic hypothesis,* in which the psychological value of metaphysical representation is very clearly shown.

We do not purpose to discuss here the antinomies which gather around the hypothesis of the atom. Since we cannot represent to ourselves a portion of matter without endowing it with all the attributes of matter, the image which we form of atoms by picturing them as extremely tiny bodies, meets with a possibly insurmountable difficulty as soon as we attribute to them a real sense.

Yet we cannot deny that the new investigations founded upon the experiment of Crookes's tube, and the ideas concerning the electric explanation of the indivisibility of the atom, may perhaps remove the most serious of the difficulties to which we have just alluded. Nevertheless such difficulties would arise again as to the *electron,* so that a wisely positive mind can see in the atomic hypothesis only a subjective representation.

Robbing the atom of the concrete attributes inherent in its image, we find ourselves considering it as a mere symbol. The

logical value of the atomic theory depends then upon the establishment of a proper correspondence between the symbols which it contains and the reality which we are trying to represent.

Now if we go back to the time when the atomic theory was accepted by modern chemistry, we see that the plain atomic formulae contain only the representation of the invariable relations in the combination of simple bodies, in weight and in volume; these last being taken in relation to a well-defined gaseous state.

But, once introduced into science, the atomic phraseology suggested the extension of the meaning of the symbols, and the search in reality for facts in correspondence with this more extended conception.

The theory advances, urged on, as it were, by its metaphysical nature, or, if you wish, by the association of ideas which the concrete image of the atom carries with it.

Thus for the plain formulae we have substituted, in the chemistry of carbon compounds, structural formulae, which come to represent, thanks to the disposition or the grouping of atoms in a molecule, structural relations of the 2d degree, that is to say relations inherent in certain chemical transformations with respect to which some groups of elements have in some way an invariant character. And here, because the image of a simple molecule upon a plane does not suffice to explain, for example, the facts of isomerism, we must resort to the stereo-chemical representation of Van't Hoff.

Must we further recall the kinetic theory of gases, the facts explained by the breaking up of molecules into ions, the hypotheses suggested, for example to Van der Vaals by the view that an atom has an actual bulk? Must we point to physical phenomena of quite a different class, for example, to the coloring of the thin film forming the soap-bubble which W. Thomson has taken as the measure of the size of a molecule?

Such a résumé of results shows plainly that we cannot help the progress of science by blocking the path of a theory and looking only at its positive aspect, that is to say, at the collection of facts that it explains. The value of the theory lies rather in the hypotheses which it can suggest, by means of the psychological representation of the symbols.

We shall not draw from all this the conclusion that the atomic hypothesis ought to correspond to the extremely subtle sensations of a being resembling a perfected man. We shall not even reason

about the possibility of those imaginary sensations, in so far as they are conceived simply as an extension of our own. But we shall repeat, in regard to the atomic theory, what an illustrious master is said to have remarked as to the unity of matter: If on first examination a fact seems possible which contradicts the atomic view of things, there is a strong probability that such a fact will be disproved by experience.

Does not such a capacity of adaptation to facts, thus furnishing a model for them, perhaps denote the *positive* reality of a theory?

24. POSITIVISM IN BIOLOGY.

If several Alpine climbers find that they have reached the summit of a mountain by various winding paths, through the midst of a tangled forest, they may well forget the doubts as to the choice of path which separated them at their starting point, and rejoice together over their arrival at the long desired goal. Absorbed in the superb view which rewards their toil, these men will refrain from all discussion until the desire seizes them to climb yet higher, and forces upon them a fresh choice of road.

Precisely so the physicists, when they have succeeded in subsuming numerous types of facts under one general fact expressed by a differential equation, can also lay aside all questions as to the subjective representations which have led them to their result. Only a further progress will require an analysis of that subject.

But in biological science, there are only a few simple and general views as yet attained. The enormous complication of the facts makes such a result difficult. Psychological representations formed from scanty observations and preliminary associations, too often appear to be inadequate to their purpose. Thus to the feeling of helplessness there is easily added the notion of something vague, indeterminate, mystical, almost a secret fear which weighs down the traveler upon this dim and solitary path, and so lessens his already feeble powers.

This picture shows fairly well the state of biological studies, when these, under the influence of general philosophy, were invaded, as by a new and more vigorous breath, by the positive spirit.

However, the first manifestation of positivism, in this realm of science, consisted in removing illusory and mystical explanations, and in promoting the particularized study of facts. The patient anatomical researches already accomplished by Cuvier into the inner structure of animal organisms by means of dissection, were en-

larged by the study of marine embryology, and resulted later in the foundation of cytology, thanks to the discoveries of histological technique.

Side by side with these anatomical conquests biological chemistry and physiology progess. The first succeeds in eliminating the view of a difference in the constitution of organic and of inorganic matter while the second reduces and explains many physiological processes as physico-chemical phenomena. These particular and well certified facts seem to form the most positive part of the successes accomplished by recent biological study.

But they agree with an analytical direction of partial research which tends towards a physico-chemical explanation of vital phenomena. While pointing out the resemblance between biology and physics, Comte has already brought into strong relief their differences, thus protesting against the attempt to reduce the one to the other. And the nature of the physiological synthetic knowledge, in contradistinction to physical knowledge, has been clearly set forth by Claude Bernard as well as a new order of relations, a different grouping of the elementary phenomena, taken in a hierarchical determinism, which constitutes *physiological synthesis.*

Now this synthesis is expressed, to a certain degree, by some general representations, which appear in such a domain, as ideas that guide research. The fact that the hypothesis of evolution has assumed this office of director of anatomical and physiological studies, is due precisely to the theoretical constructions which this hypothesis has called forth for the explanation of the facts of heredity and variation, and to the view of the problems of general cytology connected with it.

We approach this field and soon see that the comparison of observation and experience, in a word, the positive study of facts in their objective content, no longer seems sufficient. Once more, upon the ruins of the old discredited theories of *animism,* of *vital force,* and of the *nisus formativus,* new representations are constructed, related to some currents of thought, which the incompleteness of knowledge formerly hindered from developing normally.[1]

While from every side we are drawn to the study of the cell, to which the problems of reproduction and of histological development lead, it is plain that positive research turns toward the most

[1] Cf. Ives Delage, *La structure du protoplasma, et les théories sur l'hérédité, et les grands problèmes de la biologie générale.* Paris, C. Reinwald, 1895.

essential anatomical characters of protoplasm especially in case of the germinal elements, and tries to associate these concrete distinctions with the differentiating characteristics of the resulting organisms.

But this road does not seem to lead directly to our goal, and an example may make things plainer. Among the most definite marks of diversity in the structure of the germ-cells is to be ranked the number of chromosomes, or parts of the nucleus distinguishable by the microscope.

This number is a constant principle of classification for every zoological species, remaining unchanged, in reproduction, in consequence of the reductive division which precedes fecundation. Now it would seem very natural, *a priori,* to seek in that number a sign, as it were, of the level reached in phylogenesis; but a simple observation of the facts shows, on the contrary, the slender importance of this number in relation to the distinguishing characteristics of the species. In fact it is enough to note that *Ascaris megalocephala,* to which the studies of the most elementary phenomena of embryogenesis refer, shows two varieties *exactly alike,* the *Ascaris univalens* and the *bivalens.* The germ-cell of the first contains *two* chromosomes, while that of the second contains *four*!

If then we try to connect an explanation of the characteristic of animals with the structure of their germ-cells, such an explanation must not be expected from what we see in the cell itself. Rather we shall need to carry our hypothesis further, by representing the cellular structure in its invisible parts.

Furthermore the manifold phenomena which unite in the protoplasm of a genus already lead us to consider its structure as enormously complex. Even the mere explanation of the movements of protoplasm has suggested to many authors various physical and chemical compounds representing it. Among the most recent investigations we will cite, first the results of Quincke and Bütschli, the latter of whom produced a substance having a honey-comb structure, by means of a mixture of oil in a solution of potassium carbonate; second that of Berthold who compares the protoplasm to an extremely complex emulsion in which certain osmotic and chemical phenomena take place; third, that of Verworn who in order to explain its contractile nature, figures protoplasm as composed of molecules capable of passing through three states, oxydizing and decomposing again suddenly. These theories bring before us images which are ingenious, if you will, but yet rather crude. Their

primitive and restricted character, besides, gives them but slight scientific value, so that, notwithstanding the curious reproduction of the karyo-kinetic figures obtained by Bütschli, few biologists are disposed to find in such ways of viewing things anything more than remarkable analogies. Few think them capable of being carried far enough to suggest the discovery of new facts. Hence the great difference which separates such theories from metaphysical ontology, whose broader representation gives the author the illusion of having gathered in for his own benefit all reality, or a domain of reality.

Far nearer to metaphysics, on the other hand, seem to be the *general theories* as to the structure of the germ-cell. These theories arise from the well-known insufficiency of physico-chemical notions as to the structure of protoplasm. For the latter notions teach us only the number or the quality of its component parts, but not the special phenomena of life which result from the disposition and the grouping of those parts.

Notwithstanding their far more backward state, these theories show in their construction a notable analogy with physical representations, an analogy only the more conspicuous amongst the modern views which tend to depart from physicism. Just as the physicists have made ethers and fluids in the likeness of solid, liquid or gaseous bodies, combining their properties according to the facts they sought to explain, so the biologists have sought to make a representation of the germ-cell after the image of the germ-cell of assemblages of unicellular animals, or of corms, or directly of the organized societies of animals.

These theorists really represent the cell as an aggregate or an organism of particles to which they are led to attribute already certain elementary properties of life. The very words which they use to denote the reciprocal action and reaction of those particles plainly indicate such a representation.

This at least is the fundamental character of the more recent theories which Delage calls micromeristic and organicistic.

They are called scientific, not metaphysical, theories, because they do not really pretend to form universal systems, but rather, they are grafted upon the atomic conception of matter, and because they do not get lost in the obscure mists of the old animism, nor in any way try to offer a transcendental explanation of life.

Some call them *positive* theories, because they do not lose sight of facts, and because they are always being adapted and trans-

formed according to observation and experience. But that adjective positive is not used by the biologists in its rigorous meaning, that is in agreement with the Comtian spirit which condemns the theories of electricity and light, and even the atomic theory itself, although those theories possess, in the Comtian sense, greater positivity.

Strictly speaking, a theory cannot be called positive, unless it consists purely of verifiable hypotheses. Yet on the other hand, how can we really picture as living beings, the *elementary physiological units* which are postulated as composing the cell; since it is the fundamental characteristic of living beings to appear as *organisms*?

What was said about the atomic hypothesis may be repeated concerning the micromeristic hypothesis. The view that an atom is a particle of real matter, meets with the difficulty of divesting that particle of certain fundamental attributes, such as divisibility. The view of a physiological element meets with a similar difficulty, that of divesting a living thing of its organic character.

In the last analysis, if the simple hypothesis of a certain physicochemical constitution does not fully explain the *life* of the protoplasm, the problem remains unchanged for the parts of the protoplasm in so far as they are conceived as living, for which reason we cannot really conceive such parts as *elements*. From the desire to avoid these difficulties arise also the inconsistencies which criticism points out in the special theories.

These observations show that we should be wrong in trying to attach a positive significance to these biological theories when we cannot find such a significance even in their sisters, the theories of physics. But this does not lessen their scientific value, as representations or models adapted to suggest verifiable hypotheses. In order to convince oneself of this it is not necessary to go deeply into the discussion of the theories proposed by such men as Spencer, Haacke, Haeckel, Darwin, Weismann, Roux etc. It suffices to give a rapid glance at the positive problems which they have suggested. How varied and interesting are these problems, in spite of the scanty allowance of previsions as yet furnished them by the theories in question!

Meanwhile every author has naturally brought into the representation of the germ-cell his own views as to the life of the living aggregates or of species. The so-called *biogenetic* law of Haeckel, by asserting a parallel between ontogenesis and phylogenesis, tends

to justify the bringing in of such views, which is almost, as it were, a condition of the fruitfulness of the images to which the above-mentioned analogies and comparisons give rise. Those who attribute the chief cause of the variation of species to the environment, are led to explain in the same fashion *epigenetically* the differentiation of protoplasm by holding the germ-cells as physiologically equivalent unities. On the other hand, those who see the chief cause of variation in the intrinsic aptitudes of the species, turn rather to the preformistic account of things, in the modern sense of the word, for their explanation.[1]

Let us look at an example of the influence of such views upon research.

The epigenetic conception, by suggesting the idea of the *isotropism of the ovum*, leads Pflüger to bring to light the action of gravity upon the development of the egg, showing by appropriate experiments with fertilized eggs of frogs, how the planes of segmentation place themselves perpendicularly to the force acting upon them. These experiments are ordinarily interpreted as proving the *isotropism of cytoplasm*. But on the contrary W. Roux maintains the *anisotropism of the nucleus* in agreement with his view tending towards preformism, and this view leads him to experiment upon the development of the egg when removed by a slow rotary motion from the action of gravity. The same view leads Chabry to perform his admirable experiments on the *Ascidians*, in which he killed some of the blastoderms in the egg and thus obtained incomplete larvae.

In their turn the conclusions which seem to be suggested by such experiments disagree with the results of other experiments as those of Driesch and Wilson, upon amphibians, echinoderms etc. in which after killing a blastoderm, the reproduction of a complete embryo has been obtained. And in support of the epigenetic view of the isotropism of the egg, we have also some observations of comparative embryology, for example the one which proves that the embryonic layers among the tunicata are equivalent (i. e., the same in every animal).

[1] The old preformationism, or evolutionism, asserted the enclosing of the germs within germs, in such a manner that the ovum or the sperm (according as they were believers in the predominance of the one or the other) should contain in embryo all the descendants, which would then only have to develop. This absurd hypothesis is now quite abandoned in consequence of the microscopic study of embryology. But the new preformationism postulates the existence in the germ of diverse elements, representatives either of the anatomical parts of the adult organism, or of the characteristics or the elementary properties of this organism.

Such apparent contradictions only serve together to increase our interest in attempts directed towards gathering such dissimilar facts into one adequate theory; and in any case the examples cited above have but an indirect relation to the theories of the constitution of the germ.

The importance of such theories is more plainly seen in the general questions of heredity.

Until a few years ago the *heredity of acquired characteristics* was generally accepted as a fact which fitted in remarkably with the epigenetic thesis. But, behold, Weismann, moved by his theoretic conception of germ-plasm, has come forward boldly to combat that theory. And Emery, accepting the foundation of this doctrine, makes it more probable by modifying (by means of the hypothesis of enzymes) the principle of non-heritability, in relation to the general effects of intoxications.

The recent controversy about this matter between Spencer and Weismann is too well known. It is not for us to pronounce judgment. But who can fail to see the importance of certain theories when they have the power to arouse so instructive a debate; when they instigate new observations and experiments, new distinctions of facts, whose objective value cannot be impeached?

We are well aware of the objection that some may bring forward! Certain general theoretic views are doubtless necessary to arouse these profitable discussions, but what has all this to do with the very minute notion of protoplasm, with which Weismann is so fascinated? Is it not clear that all the details specified by that writer, all the descriptions of invisible things which often cannot be conceived as real, are useless efforts of a fertile imagination, fleeing from the positive view of facts?

We answer: Weismann has reached certain verifiable hypotheses through a systematic construction of images, just as Maxwell foresaw certain relations of electricity and light through a theoretic model which is not itself a wholly verifiable hypothesis. Let us not attribute to these views a positive value that they do not possess; rather let us endeavor by criticism to separate the physical or biological hypotheses to be found in these views from what is purely representative or psychological. But let us not ignore the fact that such a separation generally becomes possible only *after* the theory has fulfilled its office. Above all let us not deny the importance of a method of acquiring knowledge which appears to be general in the

constructive stages of science, merely because this method has not from the beginning the requisites of positive information.

Scientific construction also is a fact, which we ought to study in its psychological reality,—a fact the perceptible value of which we should admit in so far as it in any way clears or solves positive questions.

25. PSYCHOLOGICAL POSITIVISM.

A conception which eliminates the psychological representative element from scientific theories cannot leave room for a special science of imaginary or representative images.

Therefore we ought not to wonder that the system of Comte results in a negation of psychology, the study of which he tries to reduce to a mere chapter of physiology. In vain did the more recent developments of Helmholtz, Fechner, Weber, Wundt etc. open for Comte the door to the hope of seeing the positivistic ideal realized.

The results of such researches have given us, it is true, an extremely valuable analysis of sensations. To-morrow perhaps they will undertake the study of more hidden phenomena, and the associations of individual images, the cerebral processes corresponding to productions of a higher order, will thus be made clearer. The physiological method will thus be able to give more important aid to the analysis of elementary mental facts.

But the notion of reducing psychological knowledge to such an analysis meets with a difficulty similar to that which stands in the way of reducing physiology to physics. Psychological synthesis develops other relations of these elements: The mental fashion of connecting phenomena does not correspond with the view of physiology.

Moreover beside physiological psychology there will always be room for a *psychology of observation,* which the English school, since Locke have taken in a positive sense. In the same group stands also comparative science, which following the methods called by Clifford *ejective,* is extended by Darwin, Spencer, Romanes etc., to the psychical manifestations of animals, and from this side approaches the investigation of all the elementary phenomena of life. (Psychology of Protists, by Verworn.)

But the study of the various intellectual and emotional products of the human mind, such as science, the arts, religion, language,

legal institutions etc., remains above all the proper object of psychology.

Let us suppose that the physiological mechanism of musical invention is fully elucidated; that to the rhythm correspond certain variations of nervous tone, certain changes in the circulation, anabolic and catabolic phenomena etc.; could the artistic creations of Beethoven be better understood in such a way?

Granting that the physiologist has described the totality of the cerebral acts corresponding to a logical inference, how can such knowledge help us to a better understanding of the mental process by which Newton was led to his immortal discovery? We ask an explanation of that mental process: The matter concerns certain relations for the comprehension of which we should go back to Kepler and to Galileo, and even further to the forerunners of mechanics, in the school of Alexandria. Here are brains in which at long intervals physiologically distinct phenomena have developed, and the synthesis we are seeking is found by translating those phenomena into terms of thought, and not by reducing thought to physiological terms.

Let us examine the evolution of words. Comparative philology and grammar fix the laws according to which words change as if they were living organisms. These laws belong partly to phonetic elements, partly to thought. And since great numbers of the most varied phenomena can be summed up in a general average, it does not seem unreasonable to seek in some of them the traces of permanent physiological causes.

But who could fail to see the folly of holding that the science of language will some day be reduced to the study of Broca's convolutions?

Without bringing up more examples, we think that we have made sufficiently clear the thesis that the physiological means should never be confused with the end of a psychological research; that is to say, with the *knowledge that is able to satisfy the previsions required in this domain.*

26. HISTORICAL AND SOCIOLOGICAL POSITIVISM.

The position of Comte with regard to historical and social science, is somewhat singular, since his special sociological views seem to contradict the indirect influences which result in this field from the spirit of positivism.

The tendency toward the precise determination of facts places historical research at the base of the social sciences, and transforms the method of research through a critical examination of the sources. Hence it follows that historical fact is conceived in its objectivity as independent of traditional representation, as free from whatever artistic fancy has added to it. Historical fact is conceived as restricted to the still existing reality of its material traces, which are its varied and manifold documents.

But, as against the resulting specialization of investigations, Comte has maintained here also the need of synthesis. Consistent here with the general trend of his system, he does not seem to have been equally so when he connects this synthetic principle with the *law of the three historical stages.*

To explain social progress by the development of knowledge through the successive phases: "theological, metaphysical and positive," is to superimpose upon facts a theoretical conception opposed to his tendency to eliminate systematically all theories.

The most indulgent criticism that can here be made would be that the working out of a sociological science required of Comte that he should himself pass through the "metaphysical" stage.

A later phase of the same discussion may be found in the economic doctrine now known as *historical materialism.* This doctrine, perfectly consistent with the spirit of positivism, appears as an important guide and criterion of historical research, in so far as it serves to determine a choice of the facts to be studied for certain purposes, and to determine the connection of these facts according to new and notable relations. But if, on the other hand, historical materialism is considered as a systematic view, which eliminates or neglects other historical factors, and in particular those of the ideal sort, then an avenging criticism finds its opportunity to reinstate the psychological element. As opposed to the thesis that the will which works within the social order and that the ideal ends by which it appears to be inspired, are determined by the presence of economic needs, there stands the antithesis that the economic relations are in their turn voluntarily transformed in many different ways, for instance by means of legal institutions.

One can understand legal institutions themselves in a truly historical sense only in case one considers the material factors and the legal ideas together. One must regard the resulting legal forms as a unification of the rules that seem to proceed from a supposed "perpetual will," such as aims at a "reign of equality" and such as

tends *to be extended by analogy,* so as to result in an *economy of effort* in the choice among conflicting interests.

27. THE POSITIVE ENDS THAT MAY BE PROPOSED FOR A THEORY OF SCIENCE.

In the foregoing critique we have tried to distinguish certain peculiar attitudes of the positivist school, which restrict the view of scientific reality to the objective element, whereas it seems fitting that the positive spirit, rightly understood, should consider also the subjective element.

In the field of physics and of biology the subjective element belongs to the way of representing the facts, and is of prime importance in the acquisition of knowledge.

In the realm of the psychological and social sciences, this element forms a very essential part of the fact to be explained inasmuch as in it are united directly the previsions which form the object of knowledge.

The distinction between subjective and objective has, moreover, only a relative value, and the consideration of knowledge as a whole leads us to unify the view of physical and psychological cognitions, thus forcing us to perceive a mental element even at the base of physical knowledge.

The foregoing observations have made clear the office of that criticism which tends to distinguish the subjective from the objective in science, by showing it to be a general factor in the progress of science, and we now see arising from such criticism the great problems of positive epistemology, and we can understand their aim and meaning.

First of all we should fix the standards which correspond to our conception of objective reality, that we may avoid the illusions and errors of the senses. The really positive development of such a research should lead to a wide application of the theory in the technical realm of observation and experiment. The correction of the *personal error* in geodetic and astronomical measurements affords an instructive example of this.

But side by side with the datum of the senses in the form of sensation, are found in the progress of knowledge the data of mental interpretation.

The primitive conception of reality enlarges and completes itself; the crude fact evolves, assuming the characteristics of scientific fact. What is the form of this process, and how and to what

extent can the subjective and objective elements be distinguished in that process? What then remains that is arbitrary in science, and what meaning should be attached to this arbitrary element?

The analysis of fact will lead us first to this general result, that concepts are an important agent in the acquirement of scientific knowledge. And such an analysis will give us some fundamental standards by which we may judge scientific theories

Now in the formation of concepts, we shall see not only an economy of thought in accordance with the views of Mach, but also a somewhat determinate mental process, the study of which will lead us to discuss on the one hand the *problems of logic,* and on the other hand the *acquisition of the more general concepts* of geometry and mechanics, their actual significance and their progressive extension. Now the *first problems of science* should form, as we understand the matter, an *introduction* to positive epistemology.

We also wish to demonstrate the two ways in which such an epistemological science can be considered. For it may be viewed from the logical or from the psychological point of view.

From the first view-point, peculiar to the science of nature, our criticism turns to reality, and aims at investigating it as an object of knowledge independent of the causes of error which belong to the weakness of our intellect. Epistemology thus understood completes logic, properly so called, the significance of which should be restricted according to a rigorous interpretation of the word, in order that the study of the formal coherence of reasoning shall not cloud our view of the empirical bases of the knowledge of facts.

Wherever logic is allowed to furnish the rigid scheme of demonstration and of definition, there is room for a fuller research into the process by which the raw material of sensations may be subordinated to such a scheme. For the rest, a purely logical study may be summed up in a formal judgment, which rejects mistaken deductions, and refuses to reason upon ill-defined concepts. On the other hand, the field which we are considering is that of epistemological criticism for which every scientific procedure, imperfect as it may be, represents an effort, and usually a step toward reality, and has in this sense a value that is not to be neglected.

Let us say more plainly that logic indicates the ideal method of scientific construction, while positive epistemology points out its actual method. Only the methods of proof, and as we think, only of formal or analytical proof, fall under the dominion of the former, while to the latter belong also the methods of discovery.

In this connection we should consider the theory of knowledge in its psychological aspect. The workings of the human mind, whether or no the results correspond with reality, ought to be the object of a special study, which should find, amongst the variable elements, the subjective data contained in the representation of facts, and should thus throw light upon the mental process of knowledge.

These two forms of research, the logical and the epistemological, are bound together and converge toward a common end in the service of science. This end is a more complete distinction of the subjective from the objective in the different classes of knowledge. There will result a comparative judgment of scientific theories in relation to the established facts which they explain and sum up, and in relation to the discoveries which they may be able to suggest.

Of all modern scientific men, Helmholtz seems to have had the clearest insight into the office which epistemology ought to fulfil i the service of science. He is entitled to fame for having declared that the discussion of all sorts of scientific questions leads to epistemological problems. But we cannot easily judge of the usefulness of treating these problems in a general sense, until the positive theory of knowledge, freed from philosophic controversies, shall have been built up by the united work of all students of science.

Only then shall we see how much light the discussion of a scientific theory can give us for judging of another theory referring to a different realm of facts. This statement is well exemplified when we consider how, even more than by the study of the *results* of physics, biology is aided by a critique of the methods employed to develop it and of the content of physical theories, when seen from the epistemological point of view.

Meanwhile the importance of such researches is shown by what has been already done along those lines. Above all some positive results of criticism are emerging in an ever clearer light, through the work of thinkers who tend to free their results more and more from the vagueness that characterizes earlier philosophical speculations, in which the germs of these results may be seen. A comforting promise for him who devotes his energies to this kind of investigation!

28. METHODS: HISTORICAL, PSYCHOLOGICAL, AND SCIENTIFIC.

In order that the effect of epistemology on all branches of knowledge may become more direct and active by conforming to

the needs of progress, we ought carefully to distinguish by the strictest criticism, the particular object of epistemological science from the manifold objects which belong to philosophy, taken in its widest sense.

The study of the problems of knowledge may be approached in three ways: First by means of the history of thought, as is now the usual practice in our country.

But this method cannot be really fruitful unless we follow out the study of ideas, with reference to the development of their special consequences in the field of the separate sciences

We have already referred to our conception of philosophy, as a tendency of the human mind toward unity and generality in the realm of knowledge and in the realm of purposes. And we have observed that this tendency is illustrated equally in the constructive procedure of science and in the poetical dreams of fancy, when not held in check by a lively sense of reality. For the rest there is no sharp distinction between these domains, so that the birth of science is not very different from one of those half-dreams that come just before awakening.

Thus we see the danger of an historical preparation which deals only with the vague aspect of philosophy. A sound mind is easily led by this road to that form of skepticism which grants an equal value to all ideas, if only they shall have some sort of inner coherence of thought.

This point of view may be well enough for history, for which all philosophy is but the sign of a tendency of the human mind, thus constituting in itself an interesting object of study. But this point of view is dangerous for science, which needs above all to distinguish truth from error. There is a danger of extinguishing that life-giving flame which kindles in the philosopher the love of discovery.

But yet, apart from the danger just now pointed out, various examples show how, in the study of the problems of knowledge, a mere sight of the general ideas discussed in the field of philosophy proves inadequate. And therefore the history of philosophy, in order really to help toward the solution of the epistemological problems, ought to be completed by the history of science, in the sense in which that history is conceived by those who follow the development of thought and the series of discoveries, beyond the life of the discoverers.

For example, suppose the topic is the importance of the dis-

tinction between *quantity* and *quality*. For Kant this distinction is subjectively established *a priori*. Stuart Mill has taken up the same concept, putting it, as I think, in a clearer form. For him, differences arising from different kinds of perceptions are not susceptible of quantitative comparison. That is to say they result in mental states of perception which are qualitatively irreducible.

In this sense heat cannot be qualitatively reduced to motion, nor electricity to light.

In what relation do such affirmations stand to the Cartesian principle that everything in the physical world is explained by extension and motion?

The examination of that philosophical controversy can lead only to this general conclusion: That, however qualitative differences are made to correspond to quantitative differences occurring in one same process, a complete knowledge of the facts cannot be reduced to such an explanation. For example, the knowledge of optical phenomena cannot be exhausted in that chapter on optics which embodies the mechanical theory of light taken in its strict sense.

But in positive science the question is stated in a different way. The question is whether, in any way, the qualitatively different phenomena of the physical world can be represented by one single process which shall serve as their model, and in which we need only consider differences of quantity. (Cf. Chap. VI.)

Let us continue our review of the methods which relate to our purpose.

In a certain sense, the historical method may be contrasted with the psycho-physiological method as understood by Helmholtz.

To consider the development of knowledge no longer in the series of phenomena shown by history, but rather through the study of the organs of sensation and of thought, this seems to form the direct approach to the questions of epistemology.

But we have already pointed out the difficulty of such a physiological investigation, and without undervaluing what has been gained by the analysis of the sensations, we still insist that there is another kind of synthetic connection, in which it is of first importance to consider mental objects, especially those to which our study refers.

With regard to these connections even the psychological method of the doctrine of evolution seems insufficient, at least as understood by Spencer, to furnish a true explanation of the problems of knowledge. This insufficiency results not so much from the ex-

clusively epigenetic views of this philosophy, as from the special character of the very questions of which he treats.

The progress of knowledge, although it is founded upon a continuous development, has a qualitative meaning which is discontinuous. That is, at a certain point of mental evolution, a certain condition of thought may acquire for knowledge a value that it never before possessed. Moreover the psychology of evolution, which studies simpler phenomena, is far from the special object of epistemology, which undertakes to explain the process by which the most advanced science is built up.

A third and very fruitful method for the study of the problems of knowledge, consists of the direct critical examination of science, regarded in itself as the fact to be explained. This method of observation and comparison is similar to that of the natural sciences. It is better, in short, to consider knowledge in its highest degree of development, rather than popular knowledge.

The botanist, when he finds it hard to distinguish the structural parts in the embryo, has only to let the plant grow, in order to see those parts clearly differentiated. Just so the scientific development of concepts shows the importance of the elements of both the physical and psychological orders from which the formation of these concepts has resulted.

This first appeared clearly to the present writer in geometry, where he saw three branches of this science springing from the common stem and showing a complete working out of the data of the different senses, from which the ideas of space originate. But this is not the place for the incidental discussion of such questions, which will form the topic of a special chapter.

Let us merely add to what we have already said about scientific methods, that it is not enough to view and arrange in one synthesis the general results of science. We must criticise them from the logical and psychological point of view so as to forward the purposes of epistemology as already defined.

Thus precisely through the lack of such criticism, Auguste Comte's positivism has failed to deal in any adequate manner with the problems of knowledge, although the renovation of philosophy by science was his great idea. From the epistemological standpoint therefore, the English positivism of Stuart Mill, of Bain, Lewes etc. seems superior, although possibly these philosophers had not so broad or so deep an insight into the special sciences.

But this conclusion does not lead us to ostracise one method, in order to give our exclusive preference to another.

The study of science, conceived as a "fact" should be aided by the teachings of history and also by the results of psychology.

But it is of the first importance that epistemology itself should be conceived as an actual positive science; that is, its students should undertake the solution of definite problems and should handle them, as far as possible, by strictly accurate methods. Let whoever will allow his fancy free play, giving himself up to metaphysical dreams. But this poetry of lofty minds, which will always arise as the expression of a need of the human soul, ought not to force its irregularities upon positive epistemology which still remains foreign to them.

There is in the latter a real object to explain, and therefore actual problems to solve, which ought not to depend upon the inconstant opinions of philosophers and upon the *social* interests that determine these opinions.

These problems can be freed from such entanglement to a certain degree, if we are allowed systematically to banish whatever pertains to the transcendental processes of the reason.

A research which aims to deal adequately with the *problems of science,* ought to fulfil this logical condition, to which every branch of the knowable is equally subordinated, if its aim is to set forth "facts" in any sense whatever.

Within these limits it seems possible that an agreement amongst all students might establish—independently of metaphysics—a positive epistemology, which ought in its turn to strengthen that scientific collaboration so much desired as the means of a higher progress.

II. FACTS AND THEORIES.

1. DREAMS AND REALITY.

AMONG the Arabian stories which tradition has handed down to us under the name of "A Thousand and One Nights," we recall one in which is narrated the strange adventure which befell a "waking dreamer."

By the orders of the Calif of Bagdad, Abou Hassan, who has been put to sleep by a narcotic, was carried to the palace and put in the place of his sovereign, who amused himself with making Abou Hassan believe that he was the Calif.

The poor fellow woke up in the middle of a great room magnificently furnished, while around him was a circle of women holding in their hands different musical instruments, upon which they were about to play, and black eunuchs gorgeously dressed, and standing in attitudes of profound respect. Glancing at the coverlet, he saw that it was brocaded with gold upon a red background, and ornamented with pearls and diamonds. Near the bed he seemed to see a garment of the same material, quite sparkling with gems, and on a cushion beside him the turban of a Calif.

At this sight Abou Hassan was exceedingly confused: "Why, I am the Calif!" he said to himself; then quickly correcting himself, "I must not make a mistake: I am dreaming." But when he closed his eyes again to sleep, the eunuchs addressed him respectfully, calling him by the name of their sovereign, "commander of the faithful."

Surprised at hearing these words, and gently soothed by the vision which they recalled to him, he now felt troubled by doubt. "Is it possible that I cannot tell whether I am dreaming or in my right senses?" Then, motioning to a lady to approach, "Here," he said, reaching his hand to her, "bite my finger, so that I may know whether I am asleep or awake." And when the lady, obeying the

order, lightly pressed his finger tip between her teeth, Abou Hassan, suddenly drawing back his hand, exclaimed, "I am really awake!"

There is in the background of this picture a truly philosophical idea. We see in the procedure of Abou Hassan the method generally used by every man who wishes to make himself clear about any impression, if he has reason to doubt that it truly corresponds to a reality.

In analogous cases, whether waking from a dream, or when the appearance of some strange thing disturbs the regular course of our expectations, we try to get out of that passive state of mind in which all kinds of illusions are cradled, by forcing ourselves to arouse, by our own will, a new sensation, and using it as a test of the doubtful impression. Finally we judge of this, by ascertaining its agreement with our previous expectations.

2. ILLUSIONS OF THE SENSES.

A few examples will bring to light the high degree of generality that belongs to the foregoing observations.

A baby one year old already takes pleasure in looking at herself in the glass. Sometimes I amuse myself by making her go behind the mirror. She expects to see the image that she saw before and is ready to grasp it. But since there is nothing there she is prevented from seizing it. Then she wants to repeat the experiment. Yet again, although she is a trifle uneasy, she tries to touch her image. But we have to begin over again, because gradually as her uneasiness grows less, she begins to take more pleasure in the game. Finally seeing that under the same conditions the same results always follow, she begins to laugh at the deception in which she no longer believes.

Travelers walking over the endless extent of a sandy desert, under a burning sun, learn to their cost, in quite a similar way, what a mirage is. By an effect of refraction, which Monge found to depend upon the extremely rarefied state of the lower strata of the atmosphere, there are sometimes distinctly seen the images, upright and inverted, of objects on the horizon. Thus to the thirsty traveler the region seems to be bounded by a great flood, every little mound of sand appearing in its image as if it were surrounded by water. But when, hastening their steps, the sufferers reach the place to which the flattering vision had lured them with sweet promises, what do they discover but the parched land and the dry sand?

In optics various analogous cases of illusion occur, which from

different points of view are of equal interest to the physicist and to the physiologist. In these cases it is always easy to discover the error by means of a trial purposely planned so as to give a decisive answer. The essential point is to vary the conditions under which the phenomena appear. This is the better accomplished if, in place of sight, we cause some other sense, for example, touch, to interpose as judge.

But one can also find the explanation of an optical illusion without leaving the field of visual experience, exactly as was shown in our former examples. It is enough in every case to study the conditions in which the image is produced, and to discover what must be changed in order to eliminate the cause of error.

For the rest, not only sight, but every one of the senses may be deceived under certain circumstances. The ear is deceived by the well-known phenomenon of the echo, a deception easy to discover by a suitable change of position.

Touch itself, the sense in which we place the blindest trust, is not without some causes of error, though fortunately they are quite rare. It will suffice to mention the very simple experiment which consists of touching the two sides of a little bullet with the fingers crossed. We seem to perceive that there are two bullets in place of only one.

3. THE CRITERION OF REALITY.

The analysis of the methods that we employ when we deal with clearing up reality, helps us to see what our belief in the reality of a fact requires. That analysis thus aids us in determining the character of the real, and hence naturally leads us to a *positive definition of reality.*

It is evident that we are not dealing in every case merely with the data of passive sensations. It is not enough *to see,* but we need to see what is *to be seen* from a certain point, or under such and such other conditions arranged at will. It is not enough *to hear,* but we need to hear what is *to be heard,* in such and such various ways according to our pleasure etc. And it is requisite that, according to a fixed grouping, all the sensations which we expect, from sight, from hearing or from touch, shall be produced in a manner predicted under certain well-known circumstances in which we place ourselves. If a single one of the sensations which belongs essentially to the fact is lacking, the failure of our expectations teaches us that our belief in the fact was erroneous. That is, *our*

belief in the reality of a thing rests upon a totality of sensations which invariably follow under certain conditions arranged at will.

And because the verification of the conditions pointed out forms in its turn the object of other sensations, the belief in reality rests, in the last analysis, upon an *associative relation* between our sensations; whether we are dealing with one of those more stable and familiar complexes of relations which we conceive as an *object,* or with one of those relations of the second grade which we view as a *relation among objects.*

We must finally bear in mind that the concept of the "associative relation between sensations" is more general than that which we purpose to define. "The correspondence of the sensations with the expectation" always constitutes the true characteristic of reality. And this gives us the positive definition of reality.

But this way of presenting things shocks us at first sight, because it does not seem to fit in with the ordinary ideas of things. We conceive reality as something existing outside of us, independently of all experience, and the agreement between certain subjective conditions and the sensations which follow them seems to us a *proof* of reality. Reality as we think would not cease *to exist in itself,* even if all communication between our minds and the external world were broken off.

Yet considering how the term existence *in itself* can be understood, we perceive that the expression is devoid of sense, unless indeed it signifies the *impotence of the will to modify the sensations that refer to reality,* without changing the conditions with which these sensations are bound up.

We have already had occasion to point out how full modern philosophy is of this great controversy touching the so-called problems of reality. The knotty point of the difficulty is in the statement of the problem itself. If reality is taken in the transcendental sense, which attributes to it a significance in and for itself, understood as being absolute, we fall into a skeptical idealism which calls up the phantasm of the unknowable before our eyes. But if we reject this transcendental meaning as void of sense, we come nearer to the phenomenalism of Mach, or to the interpretation of Berkeley's idealism given by our own G. Vailati, two views equivalent in substance. Idealism thus interpreted no longer even deserves that name, because it does not include any agnostic affirmation. It is not a denial of reality, but, according to Vailati, a *definition* of reality.

In any case, this fashion of thought does not rob the problem of reality of its positive sense, in so far as concerns "the relative distinction of the real from the unreal."

By this view of the question (which from the beginning of our speculations has appeared to us as a necessary foundation of every positive construction), our manner of defining reality is distinguished from that of Mach, in that it places the criterion of reality not simply in the "sensations" but in "sensations associated with certain voluntary acts."

For the rest the function of the will, in contradistinction to the passively received ideas, comes out in the metaphysics of Schopenhauer, and in various aspects can be recognized in the empirical tendency of the English philosophy.[1]

This recognition is founded upon the study of the phenomena of attention, and notably upon the facts revealed by physiological psychology, concerning the adaptation of the organs of sense and the functions of the motor nerves. Thus, for example, physiological optics has made clear, with regard to vision, how much belongs to the accommodation of the eyes and to their voluntary movements.

4. THE HYPOTHETICAL ELEMENT IN REALITY.

Let us try to draw our definition of reality a little closer and thus see how far it extends.

Our criterion refers directly to the verification of the real, in the very moment when that verification is completed. But the belief in something that has been perceived as real, remains in our minds as that which gives us an *invariant*, whether we are dealing with a material object or with a relation between objects. We see in this the *pre-supposition* that, if the required conditions were reproduced, we should then have reproduced the sensations which we have found to be joined with these in an *unvarying* way.

In the simplest case, this pre-supposition amounts to imagining a *permanent* verification which we believe we can adjoin to the present reality. Yet there are other real things that we think of as having an existence in time: for example, an action, a movement, etc., which can also be reproduced under determinate conditions. Here also we recognize an invariant, but in a wider sense.

But other reflections oblige us to extend still further this con-

[1] In this connection it is well to cite especially Julius Pikler: *The Psychology of the Belief in Objective Existence* (Williams & Norgate, London, 1890), a book which was brought to my attention after the publication of this work in the original Italian.

ception of reality. When we are dealing with a phenomenon which we regard as completed in time, we ask ourselves the question, independently of the fact that the phenomenon can or cannot be reproduced again, we ask, I say, whether it *really was* at a given moment. The *historical* reality of a fact, and the *invariability* of a certain relation recognized in it between the conditions and the sensations, seem to be two quite distinct questions. Is the difference really irreducible?

Let us suspend our judgment and turn to another consideration. The conditions of a fact are not always reproducible at will. Sometimes we meet with difficulties that amount to a practical impossibility. Sometimes we must wait for the verification from circumstances which do not depend upon us, or which only partly depend upon us. In these cases the belief in the fact cannot be expressed at any given moment in the form of a prediction. Reality, so to speak, is not always at our disposal. Of what value then is the supposition that it joins on to the present reality?

Let us think. The facts which belong to present reality are defined by us with reference to our will. Thus, for example, we see windows, doors, or perhaps the front of a house; trees, plants, paths or a garden etc. In fine, elements so associated as to form many objects, may be associated in another order of relations so as to form other objects, or one single object. And the same may be said in general of the relations between objects. A small number of our various mental states arise with that swift intensity which belongs to the interval of time known as the "present"; and within this interval a real thing is not isolated, but may enter into various associated groups. The existence of a real thing in the present, signifies exactly the possibility of such associations.

Now the belief in real things which we do not actually perceive has this value: it permits the establishment of new *associative relations* among the elements of our perceptions.

In order that these relations may be established, it is not even needful that there should have been already a direct verification. Such relations can be, in a more general way, *presupposed* by means of other given associations, and in so far as they correspond to something that is conceived as *invariable*, they constitute for us a hypothetical reality.

Only the method of gaining knowledge is different in the case of facts properly called hypothetical, and of facts which have actually been perceived. The association of certain sensations with the

idea of certain voluntary acts, arises in the latter case immediately from *memories,* while in the former case it results from them in a more or less *mediate* way, by means of a mental operation, which in its higher form constitutes a process of reasoning. But, apart from the method by which they are acquired, reality perceived and reality only postulated signify equally, both in the present and in the future, the *expectation of certain sensations under determinate conditions,* more or less dependent upon our will.

If it is possible to control the required conditions, we proceed to verify directly the sensations resulting from them, or to admit our error if our expectation has been disappointed. But if this is not *actually* possible, still our belief retains some value, either with regard to a future possibility, or in so far as the supposed fact implies in its turn new associative relations that can be verified, and which thus can lead to an *indirect verification* of the former fact which we were unable to verify *directly.*

We should consider the hypothesis in its broader sense, as an essential part of the reality in which we live. An enlargement of reality in this sense accompanies the development of our life of relations, through which we learn of new and distant countries that we have never visited, or persons whom we have never seen. These are relations inferred without direct verification.

5. THE REALITY OF THE PAST.

We are now in a position to answer the question, "In what sense is it possible to find an element of invariability in the historical reality of a past fact?"

The remembrance of the fact, if it has been perceived, already maintains a certain associative and determinate relation among the remembered sensations. But, independently of perception, the more or less complete reality of the fact has a significance that is carried over into the reality of the present and of the future, and, by means of indirect associations, implies certain specific expectations. According to this view it becomes possible to separate the real from the unreal in the past, and to estimate the errors of our memories. A false impression is cancelled in our world (except for certain psychological purposes) when it is forgotten; but not so a fact, which interwoven with the present by means of relations still lives, about us, in its traces.

In this sense, historical reality itself also implies the belief in a series of invariants, when this belief is taken in a broad sense as

a supposition serving as an intermediary which connects actual and future sensations.

Thus far we have assumed, even in our view of the past, that *active* attitude of the mind which keeps the eyes fixed on the present and the future. But we cannot ignore the fact that another, a *contemplative* attitude, concerning itself with the past, considers the flow of sensations, ideas, volitions, in another way. According to this truly *historical* intuition, our memories are so regrouped as to reconstruct our mental life in its time-sequence and thus to call forth again, in a *synthetic representation,* the emotions connected with that past life.

Now this historical reconstruction is of interest in regard to the problem of reality considered from a scientific point of view, in so far as such a reconstruction leads us to a better analysis of the concept of reproducible sensations or volitions.

Such a concept implies the possibility of comparing these elementary data of the present with those of the past, that is, of *recognizing* a past sensation in a present one. But the reconstruction just defined tells us that this recognition is not a judgment of identity; that at least the intensity of our sensations, and the emotional state connected with them become weakened in our memories; that in short our elementary data, taken in their relation to a state of consciousness, have an individuality characteristic of the time of their occurrence.

There seems to have been an abuse of this observation, in certain recent *antiscientific* tendencies of thought. The only legitimate consequence to be deduced from these considerations, is that sensations and volitions can be regarded as equivalent to one another even when they are individually different. One may if one chooses say, from some wider point of view, that they are merely similar, and from this point of view one may therefore also say that these data enter into the constitution of the real only as *abstract elements,* that is as representatives of certain recognizable elements which *correspond with each other* among the various groups of actual sensations and volitions.

Now, if from these considerations we should infer that the expectations contained in the knowledge of reality, and even the verifications have always the character of approximations where to the recognition of *known* sensations there is always added something new, while something is taken away from the old, we should say nothing strange, nothing opposed to the relative way of re-

garding knowledge, which positive philosophy shares with the plain man.

6. PSYCHOLOGICAL REALITY.

In psychology our hypothetical reality has a character *sui generis* which needs special mention. What does the hypothesis of the sensations, ideas, volitions etc. of others mean *for me*? And in what sense can I recognize in them some sort of reality?

When we observe in others certain expressions, actions etc., comparing them with similar expressions in our own mental states, or else trying to bring ourselves into a certain *spiritual agreement* with these states of others, so that we may react alike to like stimuli, there arises in us a particular sensation or a representation of the sensations, ideas, volitions etc. of others, which are thus associated with our conditions of observation. Such an associative coordination forms an intermediate hypothesis, with which are bound up certain previsions as to the actions of men, and as to their various expressions in special circumstances.

But the belief with which we are dealing implies something more than this purely external relation. Some elements are added to it which distinguish the psychological hypothesis from other supposed objects. That is: (1) a real emotional value of a state of consciousness belonging to others, perhaps for example the emotional value which accompanies the sensation of other people's grief etc.; (2) a determination or a limitation of our own will which results from this; a limitation comparable to, but diverse from that which comes from our inability to change the sensations given by physical reality (Section 3).

Through these characteristics psychological belief approaches our former type of belief in reality. But by virtue of the latter character, the reality "in itself" of anything psychological assumes a peculiar significance. The sensations, the ideas, the volition of others bring about a change of our will, like that which similar states in our own mental world produce upon our will itself.

How this modification comes about is a problem which we do not propose to solve here. Therefore we will not inquire how, in the light of recent investigation, the reciprocal relations between mental states and their external expression, can explain the agreement of different minds. And if we were to admit also a direct action of one mind upon another, this would be an independent hypothesis.

7. THE SOCIAL ASPECT OF REALITY.

Let us accept the sensations (ideas, volitions etc.) of others as a fundamental hypothesis. Then the conception of reality grows broader. By means of the *consensus,* the criterion of reality which we endeavored to understand, in the preceding pages, in its individual aspect, now gains a *social* significance. Precisely so, to the image of an object that we see is added the testimony of others who under the same conditions see it also. This testimony finally is transmitted with the support of still other sensations which differ according to the manner in which the testimony has been produced. In any case it has its own value if it is based upon suitable criteria, such as conform to the standards of *historical* criticism.

We do not intend to analyze the principles of historical criticism. It will suffice to point out this: Reality, conceived as a social datum, always means precisely the correspondence of sensations to conditions determined at will.

With respect to human society, taken as a whole, the field of the sensations and of the objects to which they refer, is enlarged, but this variety does not give a more solid proof of reality unless the will is correspondingly more active.

This is why we see that it is at once more difficult and easier to deceive a society, than to deceive a single man. It is more difficult when a strong interest disposes people to listen and understand, as is the case for example, in a scientific society. It is easier when, as in an excited crowd, the will to doubt is lacking, and without criticism each one allows the slightest sensations of the others to increase his own.

8. HALLUCINATIONS.

The social consensus is added to our criterion of individual reality, and reinforces the belief which is bound up with that reality, in so far as there is agreement between the one and the other, at least if we have the conditions for adequate criticism. But there is an exception which must not be passed over in silence.

The existence of victims of hallucinations disturbs our faith, though these victims are few in number, principally because an inference by analogy shows that we ourselves may be the victims of an hallucination. We ask ourselves in this case whether the criterion which we have considered as a touchstone of reality, is able to ex-

plain the error to which we are exposed, or how and why it is found wanting.

The question bristles with difficulties, because the ejective interpretation of mental facts which happen in a pathological state of mind raises doubts that are hard to clear up.

In any case we can distinguish two kinds of hallucinations; the more frequent ones which affect only one group of sensations, and complete hallucinations, which fortunately seem to be quite rare.

In the first case the agreement between the various sensations is lacking, and therefore the conditions of the subject, in relation to the object of the hallucination, are not such as correspond to the existence of a real object. It is thus practicable to judge of the errors of the senses, in case a cerebral affection does not for other reasons deprive the subject of the possibility of judgment.

Thus, for example, alcoholic patients suffering from *delirium tremens* are frequently subject to visual hallucinations. They sometimes see glasses of wine, but when they reach out to take them, they find that there is nothing there.

In the *delirium* produced by *cocaine,* the hallucinations affect the sense of touch. With *paranoiacs* there are more frequently hallucinations of hearing, which the patients connect with their delusions which cause them to form a systematically false interpretation of life.

But we should not forget that these hallucinations usually disturb the patient's mind deeply, arousing feelings of terror, of superstition or of passion. For this especial reason it seems that most victims of hallucinations, above all *hysterical* or *epileptic* patients, are incapable of making such an effort of the will as to fix their attention upon the data of the different senses and to make the comparison which might clear up the deception under which they labor.

This observation leads us to inquire how the falsely pictured fact, in case we are indeed dealing with a complete hallucination, may be brought into relation with the criterion that we commonly apply in judging of reality, so that an actual exception will no longer appear.

It really seems that a complete hallucination, in no respect distinguishable from the complex sensations produced by a real object, presupposes a mental state in which the will is dormant or, as it were, worn out. And in fact the most characteristic of complete hallucinations are seen in *hypnotic sleep*, in which the patient is completely under the control of the person who has hypnotized him.

We may say then that in such cases the first conditions for the testing of reality are lacking. The patients are unable *to doubt,* and so to submit their false impressions to a *critical proof directed by the will.*

We do not pretend to have thus made an exhaustive analysis of the questions about hallucination. This great subject, a field always open to psychological investigation, would require a more profound study than we can undertake here.

It is enough to have pointed out the difficulties which may arise from this side as to the results of our critique of the positive definition of reality, and to have indicated at the same time that these difficulties may not be insuperable.

If we succeed in proving that the subject of a complete hallucination is incapable of *willing* to test his false impressions (and this conclusion would seem to be authorized by certain theories which hold that some condition of the cortex may explain the fact), the criterion of reality already defined would be found to be inapplicable in this case, but yet it could not be said to be violated. Thus an exception is seen to be removed which would call in doubt our ideas of reality.

9. THE BIOLOGICAL VALUE OF THE BELIEF IN REALITY.

We have seen that the belief in some kind of reality always implies an hypothesis which supports our *expectations* or *previsions.*

The practical value of such an hypothesis is the fact that it furnishes a standard for the will, in so far as the latter aims at the attainment of definite purposes.

From this point of view any discussion about the legitimacy of the hypothesis seems idle, unless in so far as we examine those standards which can give us more faith in single previsions. In this sense experience teaches us how the circle of these first hypotheses can be indefinitely extended, thus contributing to make the remotest previsions possible.

Outside of such scientific experience, any general objection has no positive meaning. The conclusions of skeptical or idealistic philosophy can teach only this, that the belief in any reality cannot be inferred from another higher principle. But on the other hand such a principle would contribute nothing, because no man can seriously call in question *some* kind of reality, that is to say, regulate his actions as if he did not share the belief in reality. In fact

this belief is fundamentally necessary to life itself. We cannot renounce it as long as we live.

We have here, then, an expression of our voluntary activity, as was justly shown by the English empirical school.

But take notice! This does not in the least mean that the belief in question, as it was produced by the will, can be abolished by the will. We are dealing in fact with a primitive condition of every exercise of the will, a condition whose negation would imply the absurdity of an intrinsically contradictory will which destroys itself.

Only in particular cases can the will, moved by fear or desire, exert an influence upon belief, by inhibiting the application of those standards by which we recognize the reality or the non-reality of anything. And such an influence may even become systematic in certain classes of questions, in which the false prevision is hard to disprove. But these cases do not concern us here. In this study, inspired by the sole aim of truth, we shall avoid as far as possible all complications in which the foresight of consequences either desired or undesired might disturb the calm judgment that we need for scientific philosophy.

10. THE POSTULATE OF KNOWLEDGE.

We have attempted to analyze the concept and the value of reality, explaining them from various points of view, in the preceding paragraphs. Let us now try to sum up schematically the results of our critique, expounding our belief by means of the following postulate:

There are fixed groupings, independent of us, amongst our actual or supposed volitions on the one hand, and the sensations produced by them on the other hand, (these volitions and sensations both being understood in a certain abstract sense). These groupings correspond to what we call "the real." A real thing always implies various associated relations between series of sensations that are produced under given conditions. And by virtue of this multiplicity of relations which is capable of endless extension, the hypothesis that there is a reality is enlarged, beyond the world which can be immediately perceived by our senses. Especially by means of psychological hypotheses, reality acquires a social significance (as Comte has taught).

The real gets defined, in this way, as an *invariant in the correspondence between volition and sensation.*

The word "invariant" was first introduced in the *Algebra of linear substitution,* and has taken on a more general meaning, when used concerning any *group of transformations* whatever, in the geometrical and analytical researches of F. Klein, S. Lie, H. Poincaré, etc. Some time since the expression was carried over, in a broader sense, into science, where it is often used. Ostwald, in particular, speaks of *energy* as an invariant which is as fully entitled to be regarded as an object of knowledge as *matter* itself is.

But the view that the real is an invariant ought to be determined by carefully limiting the *group of elements* (volitions and sensations), and the *group or groups of transformations* (of time, person etc.), to which that invariant refers. Such is the object of the preceding critique, and the sense of the *definition of reality* that we have been seeking.

The postulate we have expressed belongs equally to the common sense knowledge necessary to life, and to specially scientific knowledge. Let us now try to make clear the meaning of the latter by passing from the consideration of crude facts, to which the preceding remarks refer, to the analysis of what constitutes a fact for science.

A wider application of our former postulate results then, not merely in *extending* ordinary knowledge, but also in carrying further that *approximation* which we have seen to belong to it. Thus the very concept of reality will become constantly more precise.

11. COMMON FACTS AND SCIENTIFIC FACTS.

Whoever tries to make out the differences between common facts in the ordinary sense of the word, and scientific facts, sees at once in the latter a far more plainly *conditional character.*

We have certainly recognized that all knowledge contains conditions for the production of those sensations which are bound up with it. And this is true even of the fact which exists, so to speak, individually, as a past happening.

But we can distinguish *subjective conditions* and *objective conditions.* If however we are not dealing with a rigid distinction, it is in any case useful to differentiate them according to their relative importance.

Common sense attaches itself especially to subjective conditions. It teaches us that we must *look* in order to *see,* that we must *move* in order to *touch,* etc.

In fact there is in this case also a comparison of the fact we

see, with the reality that surrounds it. But it seems that we forget everything but the means of putting ourselves in a condition to produce the expected sensations.

The opposite takes place in scientific knowledge. The fact is conceived, not in relation to ourselves, but in relation to the facts by which it is surrounded. In this case, or at least in stating the result, the subjective conditions are forgotten under which the fact is revealed to the senses, in order to take note of the objective conditions of its existence.

Such a manner of considering facts adds something to them that common sense leaves in the dark. Hence a scientific fact is made up, so to speak, of the multitude of crude facts that are contained in it. Thus a scientific truth gains in *generality* while it sums up in itself new and more extended relations.

§ 12. PHYSICAL FACTS.

We can easily make the foregoing observations clearer by giving some examples.

Let us strike with a hammer on a plate of copper. We shall find on touching it that it has grown hot.

There are two crude facts, presented to us one after the other. The precise description of the circumstances in which these things occur and of the sensations experienced, is nothing more than ordinary knowledge. It is common sense that teaches how the hammer should be handled so as to strike, or how it is most convenient to touch the metal in order to ascertain its heat. These are subjective conditions which do not enter into the scientific knowledge of the fact.

Science keeps only so much: "Metal struck with a hammer grows hot." From this statement the small details of the completed experience are omitted, but to the two raw facts is added their relation in succession, a relation conceived as invariable.

The scientific fact consists of this relation. It teaches us exactly this, that "we can experience the sensations that testify to the heating up of the plate of metal, whenever we perform the acts which produce the blow of the hammer upon it."

But if we observe that other metals, or even other bodies of any sort, are heated by percussion, and that this heating takes place whether we use a hammer or any other instrument to strike with, then we begin to give a more extended meaning to the fact, and we declare that "bodies are heated by percussion."

Thus we have eliminated all conditions irrelevant to the real relation that constitutes the fact, in order to retain only that relation which hence gains a more general significance. Scientific knowledge, understood in this way, becomes far more instructive. We regard as the object of such knowledge a systematic grouping of facts, rising by degrees to greater generality and conditionality.

What a scientific fact is appears most clearly in physics. The fall of heavy bodies, the constant temperature of fusion and of the boiling point of various substances, the reflection, the refraction and the polarization of light, electro-magnetic induction, offer so many characteristic examples of facts which are at once general and dependent upon conditions.

In the instances just mentioned we see an *invariable relation of succession or of coexistence,* according to whether we are dealing with phenomena considered from the *dynamic* or from the *static* point of view.

Other sciences will give us occasion for discussion as to whether other facts remote from this type, are capable of being reduced to it.

§ 13. FACT AND LAW.

But before we turn to this matter, we must pause over a question of terms, which involves a serious philosophical controversy.

Facts subject to conditions, of which we have lately spoken, are commonly called "laws," especially when they are stated in a simple and general way.

The name "law" suggests to the mind the idea of a voluntarily fixed relation, to which concrete or possible facts *ought* to conform. This is precisely the political significance of the word. And although the wise legislator holds by the maxim of regulating only relations that have already a basis of fact, yet we can conceive a law which should be without such a substratum. A case in point is a tale told of the ruler of an Oriental people who placed severe penalties upon duelling, although that custom was completely unknown to his subjects.

It is easy to see how such a conception of law has been superposed upon physical relations. This indeed becomes perfectly clear if we imagine a chaos, from which God calls forth the actual world, ordered according to a pre-established plan.

But, outside of this theological representation, there remains associated with the concept of a *physical law,* the idea of something

necessary, which *must* exist independently of its verification, or at least of the contingent facts, whose relations the law expresses: As for example, when we think of the laws of geometry and mechanics, as expressing certain properties of the movements of bodies, while conceiving those laws as if independent of the existence of such bodies.

The word "necessity" often receives different interpretations among philosophers, in relation to the most varied appreciative judgments. What does this word express?

First of all, every fact, in so far as it contains an invariable relation of coexistence or of succession, gives us a certain *feeling of necessity,* which insists that we cannot voluntarily forbid the consequence when we have willed the premises.

However, our power over the realization of the premises is, in a certain sense, the greater according as the fact is more particular. In this sense general facts are more withdrawn from the control of our will, and hence they appear to us as accompanied by a stronger feeling of necessity.

But the feeling of the necessity of a fact has not only this meaning, as limiting our will. It is especially interesting to epistemology, in so far as it is accompanied by the illusion that certain representations constructed *a posteriori,* upon the basis of preliminary observations or experiments, have an *a priori* value, and hence may be extended without limit beyond their own domain. It is therefore important to notice that such a necessity of psychological order remains always a feeling whose value is purely subjective, that is, it cannot be called upon to prove the objective existence of anything.

The history of science shows significant instances concerning this point. Let it suffice to cite as an example the explanation which, before Torricelli, was given as to the rising of water in a pump, a fact regarded as necessary by whomever assumed that nature abhorred a vacuum. That necessity became less pronounced, notwithstanding that abhorrence when men undertook to overwork nature by making pumps send water more than ten meters high.

The lesson that is taught by such examples, becomes inductively clear even in relation to more general ideas, inasmuch as we find a strong presumption that the conceptions which coordinate many elementary obeservations, shaped by a long psychological working over, are well *adapted* to the totality of facts, at least in a certain field. It may be further said that the objective value of the knowl-

edge contained in such concepts, may be established, in the last analysis, only by experience, which ought in particular to limit the field of applicability of such concepts, and measure their degree of exactitude, by means of a comparison of the previsions which depend upon them.

A fuller examination of the question may be found in chapters III, IV, and V. It must suffice here to conclude that we cannot recognize a philosophical foundation for the distinction between "facts" and "laws," nor can we accept the view of A. Naville,[1] that there are *theorematic sciences* which, independently of the content, busy themselves with interweaving "the web of reality." A law "independent of content" is a transcendental abstraction repugnant to the spirit of our critique.

Let us return to the analysis of scientific fact, in order to see how certain facts, apparently widely sundered from the type treated of by physics, yet admit of being reduced to that type.

§ 14. ASTRONOMICAL FACTS.

If scientific facts are to be distinguished from "common facts" by a higher degree of conditionality and generality, it becomes difficult to consider certain discoveries as "scientific," whose interest is still undeniable and whose aim is to establish the simple existence of new objects.

Without leaving the field of the physical sciences, astronomy and chemistry offer instructive examples in this connection.

Can we fail to recognize the importance of the discovery of a star that has never yet been seen in the sky? Yet such a discovery, if the star is one whose coordinates have been established, does not teach much more than the subjective conditions required for the sight of a new object.

To understand the place that facts of this nature should take in science, it will help us to observe that every objective relation presupposes crude facts to which it refers. Scientific knowledge then requires a substratum of every-day judgments, which may be left without explicit mention, only in case they fall frequently within ordinary experience, by reason of the ease with which they may be observed. Just so the physicist, since many objects made of iron are within his reach, need not take the trouble to make an inventory of them.

[1] *Nouvelle classification des sciences*, Paris, Alcan, 1901. Cf. the critique of G. Vailati in the *Rivista di Biologia generale*, July, 1902.

But when we are dealing with objects which are somewhat difficult to perceive, because of their scarcity, or for some other reason, it is interesting for science to bring them to notice. Scientific research is in this case obliged to perform, on its own account, a preliminary operation, such as in other cases has been already well enough accomplished.

By means of this explanation we want to make clear that while the discovery of a new star is really an important result for astronomy, still it does not yet form a part of a genuinely scientific knowledge. To be sure the matter is but one of degree. In fact the interest of such a discovery consists in its having enlarged the field of crude facts, whose relations form the more proper object of science.

From this point of view the value of such a discovery is reduced to its proper measure. It is also seen how this value keeps on diminishing rapidly, as it becomes easier to extend our field of observations by means of more perfect instruments.

Still there is room for the observation of an important distinction between the discovery of a star and that of a planet.

In the latter case we may recognize in the single fact the characteristics of the scientific type of knowledge.

Let us see, for example, what Herschel taught us by his discovery of Uranus. Various observers, before him, had found that star in the telescopic field of view, so that it had often been catalogued among the fixed stars. Connecting this star with the planetary system, Herschel was obliged to follow its course around the sun, and hence to determine the objective conditions under which the observation could be repeated.

These conditions, or if you prefer, the relations of position connecting the star with the sun, form really the new fact, established by the discovery of the planet.

At the time of which we speak, another interest was added to the discovery, relating to the confirmation or the modification of the Newtonian theory which we might expect therefrom.

The astronomers, full of faith in the law which had explained known facts so precisely, saw with the greatest astonishment that this new planet did not fit in with the previsions based upon the law. They had to admit an exception which destroyed the rule, or have recourse to the hypothesis of another unknown planet, whose disturbing action should explain the departure of Uranus from its elliptical orbit.

This idea, set forth by Arago, was subjected to calculation by Le Verrier. The so-called empirical law of Bode (which is here shown to be somewhat inexact) afforded the means of making the problem determinate. And the discovery of Neptune, which by a happy combination was found to be very near the place assigned to it beforehand by Le Verrier, was hailed as a triumph of planetary astronomy, because it actually confirmed very brilliantly the reality of the extremely simple relations summed up in Newton's law.

§ 15. CHEMICAL FACTS.

Terrestrial science also offers examples of facts whose recognition means only the discovery of some new object.

Passing over geography, which suggests no observations different from the foregoing, let us see what the addition of a new substance to the catalogue of the elements means to chemistry.

There is no doubt that this must be a fact of the greatest importance. But we should be wrong to consider such a discovery as the acquisition of a mere simple fact and to judge of its interest only in relation to the small number of known elements.

First of all such knowledge tells us that it has not been possible to reduce or decompose certain substances beyond a certain limit, so that with respect to a certain group of reactions the element is irreducible. But the same knowledge is also to be regarded, in a positive and not merely in a negative sense as the basis for a series of relations between the various compounds that contain the element in question.

But when the discovery follows a prevision established upon a basis of a knowledge of more extended relations, it verifies a more general fact, in which we expect to find the characteristics of scientific truth in its higher meaning. Thus may we speak in part (taking account of the difficulty of bringing the most recently discovered substances into the theory) about the discovery of Scandium, Gallium and Germanium, which have come to fill a gap in the periodic system of Mendeleieff.

§ 16. FACTS OF THE BIOLOGICAL SCIENCES.

The aim assigned to the biological sciences is to classify systematically the animals and plants on the earth's surface. It would seem then that we thus have a whole branch of science which does not propose in any way to go beyond the recognition of crude facts.

To show how false is this conception, we observe at once that the discovery of a new species has never been regarded from the same point of view by the collector and by the man of science.

To the latter it matters little to add a name to the already enormous catalogue of animal or vegetable species. He seeks rather to determine, in each case, the constant grouping of characteristics which constitutes the type of a species.

Thus a true naturalist, unless he is pursuing some special ends, will take little trouble to bring to light minute details of structure, by which an individual is distinguished from others of his own species. Neither will he attach too much importance to the distinctions between varieties and subvarieties which may be multiplied at will.

The recognition of a fixed group of zoological or botanical characteristics really constitutes the scientific knowledge of a determinate fact. Between this and the physical fact perhaps the only difference is the greater consideration given to the relations of coexistence in contrast with the relations of succession.

For the rest, we have still a hierarchy of facts of this nature rising by degrees to a greater generality. Thus for example, from the precise description of the nervous systems belonging to various species of annelids or of insects, we get the general notion of that system made up of rings of the œsophagus and of the chain of ganglia, which is the type of the nervous system for most invertebrate animals.

But in the biological sciences, we study not merely fixed groups of characteristics, that is statistics, but also true relations of succession, analogous to those with which physics mostly deals. And there is no doubt that knowledge of this sort is of very great importance.

Dynamic facts are already met with in the study of morphology, even when limited to the scope of the systematic branches. Thus for example in zoology, when trying to classify the various cases of *incomplete hermaphroditism* (*dicogamia*) we are led to consider, besides the dicogamia allied with anatomical conditions, as seen in the lombrici, in the leeches or in the aplisie, we are led to consider, I say, the dicogamia joined with conditions of time, that is to say dependent upon the different time needed for the maturing of the ova and of the zoosperms (dicogamia *proterogina* of the *salpe,* and dicogamia *proterandra* of the *crostacei isopodi* belonging to the family of the *cimotoidi*).

For the rest we may say, in a general way, that the systematic classification of the biological sciences does not rest solely upon the study of static anatomy, but also upon dynamic anatomy, for example the *metamorphoses*, and especially upon embryology. Suffice it to recall that the most correct distinction between the higher animals and the higher plants (*metazoa* and metaphyta) is given by locating the stage of development of the *gastrula*, in the embryonic development of the former.

It follows that the organic sciences do not complete their whole task by classifying, so that many extremely important researches, belonging to the various modes of generation, amphigonic, parthenogenetic, agamic, of the higher animals, already seem to go beyond that limit.

The discovery of alternating generation, divided then into *metagenesis* and *heterogonia*, was rightly considered most interesting by the naturalists of the first half of the last century. This discovery certainly is an advance in scientific knowledge, since it brings to light various kinds of periodicity in the life of animals.

Now every result concerning generation brings out some relation of general interest, whether we are considering for example, infantile generation (*neotenia* and *pedogenesis*) or the singular phenomenon observed by Chun among the *ctenophore*, consisting of the succession of two periods of fecundity interrupted by a metamorphosis (*dissogonia*).

Finally, through the impulse given by the theory of evolution, the natural sciences have enormously extended the research into dynamic relations, proposing as they do to reconstruct the process of the transformation of species.

By means of examples taken from zoology, we have sought to characterize such facts as fall within the limits of the organic sciences. But the view of these sciences would surely remain incomplete if they were not considered in their connection with other branches of the science of life.

Thus, for example, the distinction established between systematic zoology and animal physiology is occasioned merely by the need of a division of labor. We cannot in fact get an adequate idea of the one without considering the other. While the two classes of facts are closely interwoven, an exclusive classification facilitates physiological research, and physiological study offers new standards whereby our classification may be corrected, even if at

this actual moment standards of this sort happen to be a little neglected.

In spite of the inevitable obstacles of the varied technique of observation and experiment, only a single class of facts is offered to the view of the investigator who studies the science of life, and in these facts, the further scientific research goes, the better can the type be recognized which we have already examined in dealing with physics.

Thus, through the impulse given by this enlarged conception, a great change is being brought about to-day in the conduct of biological study, and science is turning toward the examination of more general and determinate relations. In this tendency we see a promise of tremendous progress, if the separate workers, looking with clear mental vision beyond the problems suggested by their special branch of technical study, shall join their efforts for one common end! The marine laboratories, which contain great numbers of invertebrates as yet little known, or only morphologically described, may find an answer to so many problems of physiology, by studying these in simpler and more varied organs!

§ 17. HISTORICAL FACTS.

Between the scientific type of fact and the historical type some claim to see an irreducible difference which others on the contrary try to bridge over.

A word of explanation is needed in any case. History is not a collection of raw facts, since the single chance event only acquires an historical interest through its relation to other events.

But, as we have already noticed, such a relation can be examined in two different ways, either according to a purely *historical intuition,* or according to a *scientific intuition.* Therefore there are two ways of regarding the knowledge of the past, two different classes in which we can arrange such material, corresponding to aims which in our opinion are both legitimate. The question recently so much discussed, as to whether history is an art or a science, arises from precisely this difference in the point of view, and it cannot be solved except by a choice. Now such a choice merely implies the affirmation of an interest, and cannot entirely exclude the legitimacy of another interest, which may well stand beside the first, but not in opposition to it.

Now, the facts of the past, especially those concerning our social human life, may be grouped in a synthesis which aims at the recon-

struction of the past moment, so much the more perfect, according as it is fuller of those particular traits which give to the moment itself, or to the persons acting at that moment, their characteristic physiognomy. In this sense an *artistic representation* becomes our object, either for a purely contemplative purpose, or in view of the effect it may have upon the feelings of men.

We have no objection to make to those who, like our own B. Croce, maintain that this is the proper meaning of history, and it seems clear that the question of the *truth* of historical facts is not opposed to such an artistic conception. This question of truth appears either as a requisite for the extension of the proposed representation without plunging into contradictions, or as a special requisite for the *interest* of the representation itself, that is, as an element of its emotional value. While the poet, undisturbed by doubt, listens to the voice which arises from the dark dungeon of the great castle at Ferrara, telling the story of Parisina and of Ugo, the restless historian searches the documents, to find, if possible, in them some trace which may serve to confirm or deny the pious legend.

But apart from the value of historical reconstruction as an art, there remains for the conceptions of science much value in a history which, generously including all the facts of the past, and even such as for example those of geology, which lie beyond the control of man, attempts to discover their relations, in a way that may render previsions possible.

The proper character of such a view should be made clear by determining the limits and the nature of the previsions indicated, especially with regard to human history. If, for example, we should attempt to reduce history to a mere preparatory school for certain sciences, such as philology, political economy etc., or if we should try to turn it directly into a sociological science, which from the study of the past casts the horoscope of future society, if, I say, we should thus try to determine the office of history, its devotees would protest against this way of considering things.

They would tell us for example that the recognition of certain general laws which constitute the object of philology, cannot take the place of the study of the particular conditions according to which the transformation or the development of a given language has been attained in a concrete instance. And just so, the abstract knowledge of certain general economical relations, is quite unable

to take the place of the historical knowledge of a class of social facts, which however may give us an explanation of those relations. Still less can sociological laws, even if we grant their existence, teach us anything definite concerning an epoch or an environment, because certain connections though very general in space and time, are very far from determining the changeable and multiform reality.

If then we attribute to history the scientific value of prevision, it cannot as a rule deal with a well determined prevision of future history.

But the students of the past estimate differently the office of the relatively general knowledge, acquired by means of a comparison of the documents belonging to a period or an environment. They value that knowledge as a means of interpretation and correction of the documents themselves, and are thus enabled by means of suitable hypotheses to supplement the raw facts already acquired, and to understand them in their true meaning. Historical knowledge then has a scientific value in so far as it serves in a certain sense to foresee what may be the results of researches concerning the past itself, that is to say, in so far as such knowledge is capable of directing further historical research.

Whatever may be the more remote scientific or artistic aim for which such research may be carried on, the aim of progressively reconstructing the reality of the past, already suffices to give history a scientific character. For from this point of view the value of each bit of knowledge is measured by its usefulness in guiding investigation, and it follows that the relatively general relation (within sufficiently determined limits) interests us more than the separate facts, and independently of the emotional elements connected with them. Thus in the history of architecture we seek systematically, beyond the special characteristics of individual works, those by which we may recognize and distinguish a school, as for example the Byzantine or the Gothic-French. Thus also in the political history of Rome, beyond the single events of the chances of war, through which it passed from a republic to an empire, a thorough analysis brings to light certain factors of a general order which explain this transformation, and apart from the particular mode of operation, show this change to be the result of general causes. Let us mention as an example the conferring of Roman citizenship, extended to the Italian allies in the year 89, while Lucius Cæsar was consul,—an extension of rights which destroyed

the working of a form of government founded upon the direct participation of the citizens in the management of public affairs. And in the modern history of England when compared with the history of other countries, the gradual growth of liberalism and democracy is illuminated and explained by the traditional unity between the people and the aristocracy in their bearing towards their monarch.

Side by side with its artistic development, and independently of it, the development of history in the scientific sense is progressing at the present time, and directing research toward more and more profound and general relations. The investigation of economic conditions so systematically carried out to-day, is an expression of this tendency, since it seeks to penetrate, below what is more obvious or artistically interesting, to the deeper causes of facts, a proceeding which may show the facts in a new light, and thus aid in a fuller reconstruction of the past.

In this sense the type of historical fact approaches the scientific type. Historical, like physical reality grows vast and distant from us by the use of hypotheses, and tends to appear as a chain of relations, a series, as it were, of *invariants determined by the nature of the sources*, proceeding toward the abstract, and arranged in a determinate order.

But whoever tries to follow this parallel to its last consequences, must meet with a difficulty peculiar to human history, but which does not exist in other branches of historical science, as geology for example.

In physical research we are encouraged by the confidence that we can continually extend our knowledge of causal conditions, without necessary hindrance. This view cannot be carried over unaltered into the field of history, because, even if we admit determinism in its broadest sense, *historical determinism* does not completely follow from it, in the sense that all the facts of social human life can be presented as the consequences of social causes which can be discovered by thorough study. Plainly an *historical explanation* in this sense may meet with circumstances, such as the death of a man or the coming on of a storm at such a moment that it decides a battle, events which in the history of society are considered to be accidental. For such reasons Cournot's so-called *theory of chance* seems irrefutable when on such a basis as this it states that there is a limit to historical determinism.

To estimate the importance of these accidental events, to point

out, in fine, the limits of historical determinism, seems to-day to be still the great problem of the philosophy of history.

§ 18. HYPOTHESIS IN ITS RELATION TO SCIENTIFIC KNOWLEDGE.

We have attempted to connect with the type of scientific fact as exemplified by physics, some classes of facts that seem rather remote from this type. Let us now return to the type itself, regarding it however, if you will, in its most perfect expression, in order to develop some reflections of a general sort.

In the first place, scientific fact whose nature we have sketched, seems to be always progressing, rising gradually to a higher degree of conditionality and generality, and thus coordinating a greater number of crude facts.

But another circumstance now attracts our attention. In this progress we get always farther away from the reality which falls immediately under the notice of our senses. The scope of the hypotheses becomes far larger than is required by the ordinary needs of life. The realm of knowledge is extended to a multitude of facts which it becomes practically impossible to verify, but which still hold together like the links of a chain whose ends we hold.

Only habit hinders us from noticing how much hypothesis there is in some facts which we always conceive as the object of direct observation.

We cannot imagine any thing more evident than the existence of a sun made up of a glowing mass. Yet this affirmation of astronomy supposes a whole series of unverifiable sensations. So true is this that the fact to-day regarded as the object of an observation within the reach of all, was not admitted in a civilized epoch rather near our own. Thus the Greeks, whom we consider as a model of high culture, rose up against Anaxagoras the philosopher who so monstrously materialized the god Apollo, and dared to see in the sun a burning body *larger than the Peloponnesus*!

Let us examine the significance of the hypotheses contained in scientific knowledge.

To make an hypothesis signifies:

1. To expect or to foresee given sensations under certain future conditions.

2. To arrange among the groups of actual or controllable sensations, an intermediate grouping which shall serve to associate them in a given order of prevision.

The first signification relates to the field of experiences that are conceived as *possible*. To fix the limits of this field is a delicate question which depends, at least partly, upon the state of knowledge, because certain difficult conditions which to-day seem impossible of control, may be realized to-morrow.

In any case, apart from *concrete* possibility, we admit as *ideally* possible, such hypotheses as imply a *simple extension* of the *conditions* under which the experiment can actually be carried out, or of the *known sensations* that correspond to it. In this way we speak of *distant facts,* similar to those that we verify when near, and of *very large* or *very small* objects, in which cases the given objects of perception are supposed to be merely multiplied or divided, without essential change of nature. Certain instruments, as the telescope, the microscope etc., give a concrete meaning to the extension of such sensations.

In the second signification, the possibility of forming an hypothesis is not in any way limited by the impossibility of realizing the required conditions. Instead of the *direct* verification we have then an *indirect* verification that can be carried on without limit, so long as we are dealing with supposed facts to which there corresponds a description that one could conceive as completely realized without *necessary* hindrance.

In the process of gaining knowledge, but not in positive science when completed, there will be room to consider, besides the foregoing hypotheses, still other purely ideal hypotheses. In the latter the conditions and the sensations associated with them are not merely extended beyond their actual practical limits, but are even *changed in their nature.* Thus these hypotheses imply an abstraction from some of the data that experience furnishes in such cases. And so there is a hindrance in the way of conceiving the supposed fact. And thus the objects corresponding to these hypotheses can no longer be considered as *real,* but only as *fictitious* entities, which fulfil the office of a rather ill-defined intermediary, connecting the facts. The *mental images* of atoms, of ether etc., may serve as examples.

§ 19. THE VALUE OF SCIENTIFIC KNOWLEDGE.

Another general reflection relates to a question which has come up more than once, at least by implication, in the analysis of "fact," that is the question as to the *value* of scientific knowledge.

A thorough critique of this point would necessarily go beyond

the bounds of science, for since a scientific fact is finally seen to be the sum of crude facts, its interest appears to be dependent upon the interest of such facts, and hence it becomes material for appreciative judgments, such as may be subordinated to different standards, according to the various ends desired.

But there is a judgment of value that falls within the limits of science. It attempts to estimate general and abstract relations, by measuring their capacity for collecting and representing facts, and the *similarities* and *differences* of facts, in case the facts are already considered interesting. This is a valuation of concepts in relation to the purposes of knowledge. From this point of view the *number* of crude facts associated in a scientific fact, or the generality of the latter does not form its only value. Besides this we shall take account of a subjective element, that is, the *manner in which* the crude facts themselves are associated. For the facility of concrete prevision and the possibility of further scientific progress depend upon this manner of association.

§ 20. KNOWLEDGE BY MEANS OF CONCEPTS.

Let us look more closely at this subjective aspect of scientific knowledge. While in such knowledge there is always an increase of the actual mass of facts, which are, as it were summed up in the most general invariants, the form of our prevision progresses and becomes adapted to the various cases, thus becoming more and more indeterminate.

Therefore these general relations, taken by themselves, do not constitute mere sums of crude facts. There is in them, as we have already seen, an *abstraction from* or elimination of certain data. The scientific fact takes on the form of a *simplified fact, a type of a series of possible facts.* And the concrete prevision is accomplished by adding to the type the knowledge of certain *elements* considered as accidental, which distinguish and place a real fact in the series itself.

Now this type takes the name of *concept,* and thus we may say that *scientific knowledge usually takes the form of concepts.* This statement contains a view of the greatest importance, which may already be attributed to Aristotle, and upon which Kant has founded his "Transcendental Logic." But from this same principle it still seems possible to deduce new and fruitful consequences in relation to the theory of science.

To represent facts by means of concepts means, as we have

said, to associate and to abstract, and next to group the crude data, then to separate them, *choosing* what is to become an *essential* formative element of the concept, and rejecting the *accidental* elements.

As we have noted (§ 5), a similar selection is to be found in every-day knowledge, and we may say that it has part in the voluntary grouping of the sensations corresponding to a known fact. This parallelism may be followed further. In truth to *suppose a concept* forms as it were the *voluntary premise* of the scientific fact. For such a fact implies a choice of data, and thus an *arrangement of experiences.*

The importance of this voluntary element is plainly shown if we compare *different concepts* equally capable of accounting for a certain group of experiences, because there then results a certain *arbitrary* character of scientific knowledge. For the latter may be valued exactly in proportion to the *ease of prevision* and the *increase of its range.*

But the discussion of such questions implies that we are no longer to consider science as completed, but rather as developing further, so that in place of the acceptation of *acquired facts* we have *hypotheses* and *theories.*

Let us then treat of the acquisition of knowledge, first pointing out the historical position of the problem, and then showing how its solution is connected with that doctrine of knowledge by means of concepts which we have already stated.

§ 21. EMPIRICISM AND RATIONALISM.

The critical examination of knowledge which, after beginning with doubt, reaches an ever surer and more satisfying faith, leads us to speak of the means of scientific investigation. These are *observation, experiment* and *reasoning.*

Without entering into a detailed discussion, suffice it to say that the two former have often been opposed to the process of reasoning. The controversy between *empiricism* and *rationalism* arises from precisely this false manner of representing things.

But that controversy is already closed for one who understands the place occupied in modern science by the different methods of investigation. It is remarkable how minds of different training have come to the self-same result by various ways. Claude Bernard treats the question very happily in his classic *Introduction à la médécine expérimentale.*

To-day the claim that by reasoning we can penetrate the secrets of the universe, which should be mysteriously revealed in the laws of the mind, is banished as a chimera.

But on the other hand we see that all observations and experiments are of scientific value only in so far as they are supported by a reasoning process. Otherwise we should be obliged to wait until nature should be so obliging as to teach us, by answering at random those questions which we should neither know how to ask nor to interpret.

For the rest, if we recall how we have seen the circle of hypotheses widening as the realm of scientific reality is enlarged we shall understand how far we are from that narrow empiricism which, following the maxim of Saint Thomas the apostle, would believe only in immediate sensations.

The process of investigation appears to-day as an operation of the mind, which, starting from the sensations, returns to them by means of a shorter or longer reasoning process.

The different stages of this operation are similarly described by various authors, for example, by Bernard and by Stanley Jevons, who distinguish the *preliminary observation,* the *hypothesis,* the *deduction* and the *verification.* The latter may be either an observation or an experiment, according to whether we are merely *expecting* sensations already foreseen, or are *producing* them, by voluntarily causing a change in the phenomena we are studying.

§ 22. THE ACQUISITION OF KNOWLEDGE.

The analysis of Jevons is irrefutable from the logical point of view, and notably so in that part which treats of the place granted to the hypothesis in science. E. Naville in his interesting monograph on *La logique de l'hypothèse* has plainly shown that the most brilliant scientific and philosophical minds have recognized the importance of hypothesis.

In any case in the use of reasoning, the hypothesis is often taken for granted, so that the aspect of the procedure which is actually employed in gaining knowledge, differs from the logical scheme as defined above. And in any case, that scheme does not take account of the origin of the hypothesis, a psychological fact regarded as belonging to the divining power of genius.

Let us recall an example cited by Jevons. Torricelli discovers atmospheric pressure by starting from the observation that water

rises in a pump to the height of ten meters. By reasoning he is led to perform the experiment of the barometer.

Here the hypothesis is clearly conceived before the experiment. But in perhaps more frequent cases the hypothesis remains hidden and is hard to bring to light.

Let us recall for example the reasoning which enabled Galileo to foresee the result of the experiment showing that bodies fall to the ground with equal velocity, thus disproving the false hypothesis of a velocity in proportion to the mass. Galileo thought of two similar masses falling side by side. They will acquire an equal velocity. Then he said, let us unite the two masses; the one cannot increase the velocity of the other, so that the single mass, the sum of the two, will still move with the same velocity.

The experiment which verified this result was in Galileo's eyes a simple confirmation, which he perhaps did not think he needed for his own benefit, since the simple reasoning described made him already certain of his result.

But that reasoning process presupposes that the fall of heavy bodies takes place by means of a force which acts upon each body as if it were isolated from all others; and this hypothesis is substantially equivalent to a generalized form of the very fact we are trying to demonstrate. It seems in any case to agree best with the conception of cause drawn from inorganic nature, in which causes usually take effect without reciprocal action.

But now let us try to carry out the same line of reasoning in the realm of life! Let us apply it, for example, as we might feel authorized to do, to the class of mental phenomena. We shall thus be led to conceive the will and the behavior of a crowd as the simple result of motives acting upon the individuals of which it is composed, and we shall thus be led to establish previsions which are falsified by experience. For this concept of causality does not here agree with the facts, as we have seen clearly demonstrated by recent studies of collective psychology.

The theory according to which we tend to gain knowledge by means of concepts may be able to solve the epistemological difficulty already pointed out.

For in reality the stage of inductive reasoning which precedes deduction, is not an hypothesis framed by a mysterious divination of facts, but the concept by means of which those very facts themselves are supposed to be represented, a concept arising from preliminary observations by the often unconscious work of association

and abstraction. Every concept thus formed, contains hypotheses, but these need not be made explicit by a critique which states the results that are imagined as a consequence of the hypothesis.

In the first example cited, Torricelli subordinated the fact that water rises in a pump to the concept of the equilibrium of two fluids in communicating vases, an equilibrium which supposes that the pressure exercised by one of the fluids upon a unit surface is in proportion to the height and the density of the other fluid.

In the second example, as we have already shown, Galileo subordinated his hypothesis to the general concept of a force causing the fall of bodies, and acting upon them independently of their union.

Let us then contrast with the logical scheme of Jevons, a psychological scheme of inductive reasoning which shall include the four stages of *preliminary observation,* of the *concept hypothetically representing it,* of *deduction,* and of *verification.*

Thus we come nearer to grasping in its reality the procedure of acquiring knowledge, and to explaining the mysterious faculty of genius, to which the divine power of prophecy is attributed. And indeed we can see that a study of the origin of concepts may show how a finer sensibility and a more extended association may allow us to obtain, from a small number of observations, concepts so framed as to represent facts adequately. Since, on the other hand, we see that the first part of a reasoning process is often accomplished unconsciously, we are called upon to consider the many causes of error that accompany such previsions.

Let us notice here that the new *theories of degeneracy in connection with genius* (concerning which we are not competent to pronounce) do not seem incompatible with the concept of the qualities of genius already pointed out, since it is often shown that hyperæsthesia of certain cerebral associations is joined with a lower tone of other cerebral functions.

§ 23. SCIENTIFIC THEORIES.

The process above described, while it objectively leads to the knowledge of *scientific facts,* is seen in its subjective form as a *theory.* The name scientific theory then belongs to every concept or system of concepts, obtained by induction from preliminary observations, to which, by means of deduction, we can subordinate supposed facts more or less perfectly verified.

For the rest, it happens that the inductive and deductive phases are interwoven in their actual development, so that it only becomes

possible to reduce the procedure already described to a theory, if the latter is broken up into a series of connected theories in which the consequences of the one are accepted as the premises of the following, instead of preliminary observations.

§ 24. THE THEORY OF GRAVITATION.

Let us go back to a classic example, and try to reconstruct in psychological terms the process whereby Newton's theory of gravitation was reached.

The preliminary astronomical observations, completed by interpolation, are found expressed in the following laws of Kepler:

1. The planets describe elliptical orbits, wherein the sun occupies one focus of the ellipse.

2. The areas described by the radius vectors are proportional to the time taken in describing them.

3. The squares of the periodic times are proportional to the cubes of the major axes.

The same laws apply to the relative motions of the satellites of Jupiter and of other planets, except that the third law applies only to the case of several satellites. The first two laws in particular are valid also for the motion of the moon around the earth.

The first step of Newton's analysis consisted simply of substituting for Kepler's concept of a system of elliptical orbits with a given focus, the concept of an attracting center around which move the planets, or the satellites of a planet, regarded as points. And the idea was evidently suggested to him by a remarkable association, which carried his thought back to the cases studied by Huyghens, in which the motion of a point, revolving in a closed orbit, requires a centripetal force of attraction directed toward the center, so as to counterbalance the centrifugal force. In the case of a point tied by a string to a center, and describing a circle with uniform velocity, the force of attraction, measured by the tension of the string, is thus directed toward the center, and its measure is inversely proportional to the square of the radius.

Now this case is a special example in which the first two of Kepler's laws are found to be satisfied. A well-known deduction leads, with Huyghens, to the extension of this conclusion to the general case: a point which is moving around a center, in a closed orbit, in agreement with Kepler's second law, undergoes an acceleration directed towards the center; and if the orbit is an ellipse and

the center of rotation one of its foci, the resulting acceleration is inversely proportional to the square of the radius vector.

Hence the motion may be explained by supposing a force of attraction inversely proportional to the square of the distance, and emanating from the center, a force which is added to a given original impulse received at a certain moment by the moving point, in such a manner as to satisfy a certain inequality between the original data.

So far we have gained this, that we can divide the Keplerian hypothesis into two others:

1. The hypothesis of a force of attraction and an initial impulse.

2. That which is expressed by a certain inequality between the data of the initial state. Let us drop the latter condition. We shall now have motions that are possible in hyperbolic and in parabolic orbits. But these would be explained by Newton's concept, and so may be viewed as a direct generalization of the motion of the planets around the sun. These motions in *quasi* parabolic orbits (and perhaps in hyperbolic orbits) are found to be approximately realized in the case of comets, with which shooting stars have recently been classed (Schiapparelli).

Let us disregard this extension, and suppose that Kepler's two laws are stated with rather more generality than is usual, and let us add the third law, which brings in the attraction exerted by the sun upon the planets, and by Jupiter upon its satellites, in proportion to the masses attracted. Can we then say that the concepts in terms of which facts are explained in the theories of Newton, and of Kepler, are equivalent?

Yes, if the development of Newton's theory stopped with the logical transformation of Kepler's hypotheses, the concepts would be perfectly equivalent. Nothing more, and above all nothing different could result from them to contradict the premises.

But the value of this transformation consists in the circumstance that to the concept of Newton:

1. Other facts can be subordinated, besides the astronomical facts that were first in question.

2. There is associated with this conception a new intuition as to relations which by virtue of certain analogies and of a certain simplicity and symmetry of representation suggests *new hypotheses* that may correct those from which they took their rise.

The story tells how the fall of an apple led Newton to associate and to compare the motion of falling bodies on the earth's surface with the motion of the moon. If the attraction of the earth is exerted

not only upon the moon, but upon all bodies, inversely as the square of their distance, so that ordinary gravity seems a particular case of this law, it should be possible to deduce the value of Galileo's constant g (the acceleration due to gravity) from the acceleration of the moon, by imagining the moon to be transported to the surface of the earth. The calculation is performed, in a first approximation, by multiplying the lunar acceleration by 60^2, since the mean distance of the moon from the earth is equal to 60 times the earth's radius; and even in this first approximation we can find nearly the value of the constant g!

Up to this point the new hypotheses, which correct Kepler's system, come from representations obtained from facts, by virtue of suggestive associations.

The sun attracts the planets, the planets attract their satellites. In particular, the earth attracts the moon, and this attraction is exerted not only upon the moon, but upon all bodies which surround the earth; and the same attraction appears upon the earth's surface as gravity. The only step to be taken is a simple enlargement of the hypotheses. Every one of the heavenly bodies exerts an attraction upon all the others, always in proportion to the product of their masses, and in inverse ratio to the square of their distances.

This extension, besides removing limitations whose motive we cannot *a priori* see, arises in part from following out the analogy with the motion of a point tied to a center, in which case there corresponds to the centripetal action, an equal centrifugal reaction opposed to it. First of all Newton was led to posit in general the principle of action and reaction, and he soon deduced from this principle the fact that the planets also exert an attraction upon the sun. But this does not yet constitute a true correction of Kepler's laws, so long as they are considered as the expression of relative motion. Now why should not the same attraction be exerted by the planets upon one another? If the effect of this attraction were very noticeable, Kepler's laws would become visibly false. But since we are dealing with quite small perturbations, during not too long periods of time, it is possible to admit that the hypotheses of Kepler represent merely a first approximation, and that, adopting the hypothesis of the more generalized attraction, a more precise approximation may be obtained.

Every one knows that this has actually come to pass, and we have already had occasion to recall, among the verifications of the Newtonian theory, the brilliant discovery of Neptune.

The last step consists of extending the hypothesis of attraction to material bodies of any kind whatever. The attraction of the heavenly bodies then comes to be regarded not as a force emanating from their centers of gravity, but as the resultant of forces pertaining to their particles.

Here we have an entirely new supposition, which has been directly confirmed by the experiments of Cavendish, carried further and perfected by many investigators. We may add the indirect confirmation furnished by Airy, who measured the variations of gravity in the interior of the earth, and by Carlini, who measured the influence exerted upon gravity by neighboring mountains etc. In the realm of astronomy this extended hypothesis leads moreover to a correction of the theory, by taking account of the *form* of the heavenly bodies. In particular there corresponds to the flattening of the earth at the poles, an actual perturbation of the motion of the moon, which was determined by Laplace.

The theory which has received the name of universal gravitation, while it solves the problem of the motions in the planetary system with a degree of exactitude unsurpassed by any physical theory[1] is still being enlarged by a wonderful progress in several directions.

The relative motions of *double stars*, later observed by Herschel in the most distant regions of the heavens, conform to Newton's explanation.[2] The *form* of the heavenly bodies, and especially that of the earth, is sufficiently explained by taking as a basis the attraction of the various parts, and admitting the original fluid state of the body (the ellipsoids of Maclaurin). The hypothesis of Clairaut that the density of the earth varies in concentric ellipsoids, leads *a priori* to a quantitative estimate of the flattening of the polar axes of the earth, and we have the number

$$E = 1/292.4,$$

which agrees well with the results of geodetic measurements, which give (according to Clarke)

$$E = 1/293.5.$$

Let us add, with regard to the earth, the determination of the tides, which Newton had already connected with the attraction of

[1] Cf. especially the last chapter of the *Traité de mécanique céleste* by Tisserand (Gauthier et Villars, Paris, 1889-1896).

[2] Cf. in particular the notes of Darboux and Halphen in *Comptes rendus de l'Acad. des Sciences de Paris*, t. 84.

the moon, and as to which Laplace has drawn up concrete previsions which are useful to sailors.

Where will this development stop? The phenomena of *adhesion* and of *capillary attraction* show that at very small distances much stronger forces are exerted than those of which Newton treated. Thus we are led to a new extension of our hypothesis. We suppose that the central forces depend upon the distance according to a development in series of negative powers. The terms of the development beyond the power -2 will be negligible if we are dealing with perceptible distances.

We will not further discuss the questions that now arise, but in this connection will refer the reader to chapter VI.

Let us only notice this point, that since Newton's hypothesis, and the more general hypothesis to which we have called attention appear as equivalent in the order of astronomical approximations, we may choose between them, and thus may adopt the first formula, as the *easiest instrument of prevision* in the field of astronomy.

§ 25. A CRITIQUE OF THEORIES.

The example of Newton's theory considered in the successive stages of its development throws light especially on the following points:

1. The formation of concepts and the consecutive development of hypotheses arising from the association of various observations.

2. The abstraction from certain data and hence the increasing generality of theories. In fact, as we pass from Kepler's laws to Newton's, we eliminate first of all certain accidental constants (initial position and velocity) which can be determined in various cases by observation for the purpose of verification.

3. The subordination of new relations to a general theory by means of *auxiliary hypotheses.*

4. The *equivalence* of various systems of concepts in relation to a certain group of facts, and the development from these concepts of *different hypotheses in a more extended class of relations.*

5. The *equivalence of different hypotheses* when conceived in relation to a series of observations and experiments that are *limited to a given degree of approximation.*

From the statements 4 and 5 we obtain various ways of *simplifying theories within a certain field of previsions,* and in the second case the simplification is susceptible of a precise quantitative judgment.

Now the process of forming concepts can be greatly enlarged by adding *fictitious entities* to real objects (cf. § 18) in order to carry certain associations further, and thus to help out the imagination.

The use of such fiction cannot be limited *a priori,* but we should demand that the development of theories of this nature should be controlled by wise criticism.

Theoretic knowledge thus pursued no longer refers to a sum total of actual relations which can be verified by experience. For beside the *physical* world of experience, theory creates an imaginary *metaphysical* world, beyond all possible experience, and conceives both as equally parts of the same reality, although one is *visible* and the other *invisible.* It is the office of our critique to distinguish the hypotheses and consequences referring to these two parts. In fact only that which refers to the former has any *real sense,* that is, constitutes any actual knowledge, while the imaginary element of the theory should be regarded only as a *means of acquiring* knowledge. This means is effective precisely because the system of images is closely joined with the system of facts, but this efficacy is accompanied by the difficulty of estimating the results obtained. And so the criticism of a theory ought to lead to such a transformation of the *descriptive hypotheses* adopted as will serve to separate the *physical hypothesis* therein included, from the *metaphysical* and *optional hypotheses* which refer only to fictitious entities and imply the *various though equivalent representations of a given group of facts.*

It will be well to note at once that the equivalence of the two systems of descriptive hypotheses in relation to reality depends upon the connections ideally established between physical objects and fictitious entities, and in consequence *optional hypotheses may acquire a physical meaning in an extended theory* which subordinates a far larger group of facts to a single system.

Let us try to make these remarks clearer by the use of some examples.

§ 26. THE ELECTROSTATIC THEORY OF POISSON.

It is well known that Poisson has succeeded in explaining the phenomena of static electricity by means of a theory closely connected with Newton's system.

The word electricity arouses the sensation of a special sort of

nervous shock which follows the phenomena of an electric discharge (the spark, etc.). When we observe the conditions that precede or determine this fact, we verify the existence of attraction and repulsion between bodies placed in certain conditions (electrified), and we see in every such case forces of a higher order than those of gravitation.

The phenomena of electric attraction and discharge agree with the conception of *mutually attracting* fluids which exist in all electrified bodies, and which beyond a certain limit overcome the resistance opposed to their discharge by the bodies themselves. In order to explain these two kinds of action, attraction and repulsion, Poisson conceived *two fluids*: *positive electricity and negative electricity.*

These hypothetical descriptions can be stated as follows:

1. Bodies contain two electric fluids, the *positive* and the *negative,* which, under determinate conditions (static phenomena) are imprisoned in the bodies themselves, if they are surrounded by an insulating medium, such as a vacuum.

2. Some bodies are *good conductors,* and others are *bad conductors (insulating).* In case of the former the distribution of the electric fluids reaches a state of equilibrium almost instantly, quite independently of the manner in which the body has been charged, and depending only upon the form of the body in question and the quantity of the fluid which it contains. In the latter case the electric fluids circulate extremely slowly and may even be retained in the particles of matter.

3. In every uncharged conductor there are *equal quantities* or *masses* of the two fluids, which neutralize each other. In a charged conductor there is an excess of one fluid over the other.

4. Fluids of the same sort attract each other, while fluids of opposite sorts repel each other, and the action is for two elementary particles proportional to the product of their masses.

5. The attractive and repulsive action of the fluids is, for two elementary volumes, in inverse proportion to the square of their distance.

These hypotheses represent partly the relations between the two imaginary fluids, partly the relations between these fluids and real bodies; but it is easy to obtain from them *physical hypotheses* which express certain elementary facts.

In fact hypothesis 1 represents especially the permanence of the electric condition. Hypothesis 2 represents certain fundamental conditions of electrization by contact or by induction. Statement 5

interprets Coulomb's law, while 3 and 4 state other experimental conditions of electric action, viz.:

a. The union of two elementary bodies electrically charged exerts an action equal to their algebraic sum.

b. If two elementary bodies charged with electricity are placed in contact and then separated again, the attraction of the one increases algebraically just as much as the attraction of the other diminishes.

Hypotheses 3 and 4 contain also the *optional hypothesis* according to which the action of the positive fluid is equal and opposed to that of the negative, in the case of equal masses. In fact since we have no practical means of comparing the masses of the two fluids, the relation of the masses which neutralize each other may be taken as an arbitrary constant different from unity.

After this interpretation, what more is left in our fundamental descriptive hypothesis? What further physical consequences does this fictitious existence of electric fluids imply?

The value of our hypothesis is not very definite unless we attribute to the imaginary fluids certain properties of real fluids. It is clear that there is, *a priori* no limit to such a determination of the system of images. Let us apply the principles of mechanics to the electric fluids and suppose, for example, with Gauss that:

6. The condition of equilibrium is expressed by supposing that the potential of the electric masses with reference to each other reaches a maximum or a minimum.

By calculation we can then determine the *distribution* of the fluid in a charged conductor. This consequence follows:

Electricity is only distributed upon the surface of a conductor and not in its interior, and this superficial distribution depends, in a way which has been perfectly defined by analysis, upon the quantity of electricity, or electric mass, and upon the form of the surface of the conductor. This *theorem of distribution* is the fundamental physical fact resulting from the theory. We find different verifications of this in the *experiments of Faraday,* (which show the absence of electricity from the interior of conductors), and in the *dispersive power of points* etc.

We might well say, in a certain sense, that this theorem of distribution is equivalent to Poisson's theory, that is to say, it actually states all the positive content of that theory. Furthermore the first part of the theorem includes by itself alone the totality of the other hypotheses, if we admit the mechanical representation of electric

fluids. From the absence of electricity from the interior of the conductor (Faraday's experiments) the laws of electric attraction are in their turn deduced, and in particular Coulomb's law which in this way acquires a higher degree of approximation than it gains from direct experiment (Bertrand).

Let us now take note that the conception which serves as a base for Poisson's electrostatic theory can be modified in a very arbitrary way by constructing a series of *equivalent theories.*

We shall mention only the *hypothesis of a single fluid,* and the hypothesis of Maxwell which substitutes for the concept of masses attracting and repelling each other, that of an elastic medium placed between the electrified bodies, and exerting compression and tension upon them (cf. Chap. VI).

It is difficult to compare these various theories from the standpoint of the facility of prevision or of the practical economy of description of the facts indicated, because these are equally contained in them all. Thus our choice must be determined by other standards, that is by the adaptability of these theories to a more extended class of relations. In fact with respect to an *extension of the theory,* these descriptive hypotheses are no longer to be considered as optional. Thus for example the theory of a single fluid serves to explain certain differences between the two electric states, the positive and the negative, which are no longer considered as symmetrical.

But furthermore in two cases we have problems about facts which depend upon the descriptive hypotheses already mentioned:

a. When we try to extend the theory so as to include the explanation of electric phenomena which take place in the case of moving bodies (the time required for the propagation of electric action etc.).

b. When, without leaving the subject of static electricity, we try to estimate the *manner in which electric action varies with the variation of the medium.*

If for example we conceive the attraction of two points charged with electricity as transmitted along the straight line which joins them (the representation of the elastic thread) we are led to suppose that this attraction does not vary if the medium is charged at a point not crossed by this line.

Here then we have a *false* hypothesis which shows that the theoretic concept is not adequate to explain the facts.

These considerations lead us to favor Maxwell's theory (cf.

Chap. VI): But it will suffice here that we have shown how the determination of the fictitious entities which are added to the representation of certain facts is variable and may be progressively completed in order to serve for the further extension of knowledge.

§ 27. THE THEORY OF SOLUTIONS.

Another interesting example is offered by the *theory of solutions* which we owe to Van't Hoff. This theory is founded upon the intimate association which has been established between solutions and gases, under the concept of *diffusible* substances.

The diffusion of a gas has been well pictured in the kinetic system which regards the gas itself as an aggregate of freely moving particles which by their collisions determine pressure. The same conception answers for solutions, because the dissolving of a substance is seen to be a progressive disaggregation. It is interesting to compare this way of dissolving with the dispersion of vapors in the atmosphere.

The kinetic theory of gases (cf. Chap. VI) leads at once, in a first approximation to the following *laws of perfect gases*:

1. The *mass* is proportional to the product of the *volume* and the pressure (Boyle's law).

2. The volume is proportional to the temperature (Gay Lussac's law).

The mass, the volume and the temperature of a gas correspond closely with the quantity of the substance dissolved, with the volume of the solvent and with its temperature (density = concentration).

How shall we show the *pressure of a solution*?

When we have a gas in a receptacle which is placed in a vacuum the pressure upon the surface is the more readily manifest because we have a force whose normal action takes place at the surface, from within outward. But if this receptacle contained two gases mixed, this force would appear as the sum of the pressures of both gases, and in this case we should not be able to distinguish them. Let us however suppose that the surface of the receptacle allows one of the gases to pass through, but not the other. The former will get into equilibrium with the external environment, and the force acting upon the surface from within outward will give us the pressure of the second.

This condition may by analogy be applied to a solution. Let us undertake for this purpose an imaginary experiment.

Let us take two communicating vessels containing a liquid, water for example. This liquid will rise in both vessels to the same height, according to the law of hydrostatic equilibrium. This equilibrium will not be disturbed if we separate the two vessels by a partition through which water can penetrate.

Now there are partitions which will let water through, but which will not let dissolved substances pass through. If then we have two communicating vases full of water, in one of which there is a substance in solution and the two vases are separated by this partially permeable partition, this solution will exert upon the partition a pressure due to the moving particles of which the solution is composed, striking against the partition. How can we measure that pressure?

Let us imagine that the partition is movable, like a piston in one of these two cylindrical vases. We shall see that it becomes so displaced as to increase the volume of the solution. To hinder this displacement we must have an opposite pressure exerted upon the partition which shall precisely measure the pressure that we are trying to determine.

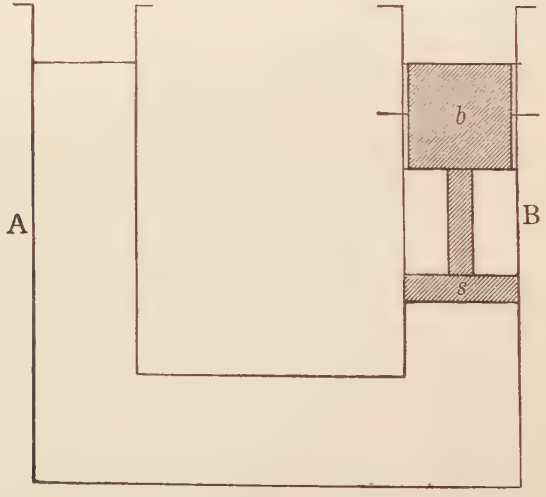

Fig. 2.

Let A and B represent the two vases. A contains the solution. Let *s* be the partition to which there is joined a rod which supports a solid cylinder *b* of a radius approximately equal to B (as shown in the figure) and of the same density as that of the water into which it is plunged. The cylinder *b* will rise partially above the

surface of the common level of water in the vases A and B, and the weight of the part above water will give us the pressure exerted by the solution upon *s*, as soon as a state of equilibrium has been reached.

But the equilibrium of the apparatus demands that the external and internal forces acting upon it should be equal.

By emerging from the water, the cylinder *b* secures the equilibrium of the internal forces. In order that the external forces should be in equilibrium, the condition expressed by the *principle of Archimedes* should be realized: the water should rise in the vase A to the same height as the base of that part of the cylinder *b* which is above water. This being presupposed, it is clear that the equilibrium of the whole apparatus will not be disturbed if we *fasten* the partition to the vase B and remove the weight of that part of the solid *b* which is above water, and which thus becomes useless.

We reach then the following conclusion:

If we have two connecting vases containing the same liquid, in one of which there is a solution, and if the two vases are separated by a partially permeable partition, the pressure of the solution, in so far as we conceive the solution itself as being like a gas, may be measured by the difference in the height of the solvent in the first and in the second vases.

The existence of a pressure understood in this way may then be expressed by the verifiable hypothesis which Pfeffer in fact confirmed, and this kind of pressure has been named osmotic pressure.

"The osmotic pressure of a solution is proportional to its concentration, and inversely proportional to its volume."

This fact which has been approximately verified by Pfeffer's experiments with dilute solutions, corresponds then to the same descriptive hypotheses that, applied to gases, give us Boyle's law.

Van't Hoff, recognizing this very law in the fact observed by Pfeffer, was led to predict that Gay-Lussac's law would also hold good for solutions. We will express it in this form:

"The osmotic pressure of a solution is proportional to its temperature," and this fact has been sufficiently verified.

In this line of reasoning we have used mechanical illustration as a means of determining what corresponds to the pressure of a gas, in the analogy that has been established between gases and solutions. The kinetic hypothesis may then, up to a certain point, be regarded as optional, if we limit its application to the theory of energy. Starting from the principle of the equivalence between

heat and work, Van't Hoff has succeeded in *explaining* the facts of the *lowering of the freezing point* and the *rising of the boiling point*, which, in the case of dilute solutions, is proportional to their concentration (the experiments of Raoult). In fact, if we refer to the second case, it will be sufficient to notice that in order to bring to the boiling point a liquid containing a substance in solution, we must furnish beyond the heat required for vaporization, a degree of heat equal to the work done in the compression of that solution.

In the successive stages of the theory of solutions, the kinetic representation occupies then a secondary place. Dalton's chemical law concerning the combining volumes of gases is extended to solutions, by virtue of the atomic theory (the hypothesis of Avogadro), and is rectified in the same way, by admitting the breaking up of molecules into *ions*.

But when we attempt to explain certain special differences between gases and solutions, a return to the kinetic descriptive hypothesis once more becomes useful, because the hypothesis of the action of the solvent upon the motion of the particles in solution leads us, with Jahn, to a correction of the laws of solutions, and we thus reach a higher degree of approximation.

§ 28. THE ECONOMY AND THE PSYCHOLOGICAL DEVELOPMENT OF THEORIES.

These examples, chosen from some of the more determinate physical theories, illustrate what we have already said in a general form, and lead us to conclude our critique of theories by a new *statement of problems*.

We have already noticed that a scientific theory may either be regarded as complete, from the *actual* standpoint, or as still developing, that is, from the *genetic* standpoint.

From the *actual* point of view the theory appears as a sum total of facts and hence of previsions. But these previsions may be obtained with more or less ease, according to the formation of concepts.

The ease of prevision is considered by E. Mach as the *purpose* of the choice which we constantly make in scientific progress between equivalent theories. Mach expresses this purpose more precisely by saying that knowledge tends to assume the *most economical* form. And the *law of economy* is regarded as a *principle of natural selection* in the strife between the different representations of the

same facts, a strife which takes place in the realm of the individual as well as in that of society.

To this grand conception, we can only add the observation, illustrated by our foregoing examples, that the criterion of economy has, in different cases, a different determinative value, and while it often requires a choice between a certain system of concept and of images, it but rarely succeeds in settling the question of a preference among different representations.

But if with the point of view of actual science we contrast the genetic point of view, the arbitrary nature of these descriptive hypotheses is connected also with the possible *extension* of theories, and thus there often results a criterion of judgment which settles the conflict.

Now from this second standpoint, scientific theory appears as a mental development, which really proceeds *by induction,* that is it obtains new hypotheses from new associations, and from the verification of these it goes on to more extended and more exact associations and hypotheses.

If we take up once more the biological comparison, in which the *life of theories* is compared with that of living organisms, we may say that the law of economy merely sets a limit to the development of theories, just as natural selection eliminates beings that are ill adapted for life. But the positive causes of the development of theories are to be sought in mental activity, and the method of this development should be explained by an examination of the laws that preside over it. *Historical causes* (which may be compared with biological *heredity*) form our first kind of explanation. But to the historical view we may add a *critique of mental processes* which may specially serve to make clear the formation and variation of concepts.

And so two classes of problems arise from these considerations: In the first place there are the problems which refer to the *deductive development* of science, that is to the *logical processes* of the transformation of concepts and hypotheses, when these processes are regarded either in their purely psychological aspect, or in their scientific applications. From the examination of these applications, there results especially the necessarily limited character of every deductive development; so that the alternation of inductive and of deductive phases which we have observed in theoretic constructions, is seen from a more general point of view to be the law of scientific development.

In close connection with the *inductive* development of science we have the second class of problems to which we have already referred. I mean those problems which attempt to explain the real meaning and the means of acquiring the *more general and abstract concepts* of geometry, mechanics etc.,—concepts to which the various special conceptions that form the object of the particular theories lead.

In this way the critique of science brings us face to face with well-defined epistemological questions.

III. THE PROBLEMS OF LOGIC.

A—PURE LOGIC.

§ 1. REAL AND FORMAL LOGIC.

In ordinary life, whoever in a discussion can make clear that his adversary's conclusions are wrong, has no hesitation in declaring him guilty of a shocking *want of logic.*

To doubt the justice of this criterion amounts to upholding the paradoxical statement that "*good* reasoning may lead to false conclusions." A *sensible* man, trying to tell us how he views this matter, would probably say that the *purpose* of reasoning is to judge of truth and falsity, and hence the *value* of the reasoning itself should be inferred from the results obtained.

As often happens, common sense is right from its own point of view. Since for common sense no problems exist outside of actual life, the answers which it suggests always have in view the direct approach to some practical end. And indeed it does approximately approach such an end, just as a straight line drawn between two points may approach a line more complex in its nature whose extremes are the same two points.

At all events the simple criterion of common sense in this case admits of easily recognized exceptions. Titus and Caius disagree about some question. After a long discussion during which each has accused the other of not knowing how to *reason,* it becomes clear that one of them believes that he has *seen* things in a certain way, while the other maintains that he has seen them in just the opposite way. At this point each of the two ceases to accuse his friend of being *illogical,* but in his heart begins to reproach him with being *blind.* And now to end the dispute both go together to the place to verify ***de visu*** what perhaps neither the one nor the other has seen.

Titus and Caius are then obliged to admit that even the reasoning of one who reasons *well* may not be *good* for establishing the truth or falsity of a fact, if even while reasoning well he has seen ill.

But common sense has not thought of such an exception, or does not remember it, and promptly begins to call any one illogical whose reasoning leads him to erroneous conclusions.

There is a frequent contrast between the synthetic view of knowledge, which aids common sense in aiming at the *purposes* of knowledge, and the analytic view of science, which after dividing the whole into its parts, treats of them as if they were isolated. The scientific result does not appear really paradoxical except in so far as it is fragmentary.

Whoever uses electric bells in his house wants the bells to ring. If they do not ring the electric system is *out of order.* There must be a break or a contact of wires, or some such cause which has already sometimes stopped the working of the bells. The mechanic comes, examines the wires which he has already carefully isolated, and finds that they are neither broken nor in contact. What good does that do? The bell does not ring.

But the water has dried out of the batteries. The mechanic only has to pour in the proper quantity, and the bell begins to ring. To obtain this result the mechanic *needed* to ascertain the cause of the *damage* which he wished to repair.

This metaphor is plain enough. The process of gaining such knowledge as tends to establish the truth is a complex operation. Reasoning is but one part of the process. Good reasoning then will not suffice to find out the truth, but other conditions must also be fulfilled. It is the task of the science of knowledge to analyze these conditions. Before reaching a synthetic view it is needful to consider the conditions, as far as possible, isolated one from another.

Can we *isolate* reasoning from its content? Here we touch upon a fundamental question much debated among philosophers: "Is logic concerned only with the *form* or also with the *matter* of the reasoning process?" And hence "has it a *real* or merely a *formal* value?"

To understand the full importance of this question on its practical side, it will suffice to consider the place occupied by logic in the scholastic philosophy. Although its studies were limited to the formal rules of deduction (or of complete induction) it was considered as an art whose object was the search for truth.

This fundamental error whose origin some claim to trace back to the very concept of Aristotle, by attributing its cause to the sources from which he drew "the art of discourse," this error, I

say, tends to be corrected in two ways by which modern thought has proceeded.

On the one hand, without losing hold of its purpose, it has been possible to enlarge the domain of logic, by including in the process of reasoning, even the interpretation of empirical data, and by seeking to fix the general rules of those associations which constitute the process of induction. Those who have made progress on this road, from Bacon to Stuart Mill, have seen the necessity of bearing in mind the *real content* of the reasoning process, and thus have departed from the ancient formulae to which the false could be quite as well accommodated as the true.

If the new *inductive logic,* while it is possessed of general principles, is far from offering us the same fixed and exact standards which belonged to the older logic, still it represents in a far more adequate way the conception of an art whose purpose is the establishment of truth.

On the other hand, while holding fast, as the object of *logic*, the *strict form* of reasoning, modern thought has been able, through Kant, Herbart etc., to show in the proper light the purely *analytical* character of this *strict form.* This juster view has been attained by attributing to the process of reasoning the value of an instrument of transformation. Such a transformation does not alter the conceptual data of knowledge, but leaves their truth or falsity to be shown by other means, while it merely establishes a connection whereby the truth or falsity of certain data implies the truth or falsity of other data.

In order that the reasoning process as thus understood shall become completely independent of actual empirical proof, and still remain rigorous, it is important that its laws shall be recognized as purely formal, that is, applicable whenever we meet with the conditions of a certain coherence of thought, regardless of its content.

In fact there is in the data of observation and experiment an indefiniteness which disappears from the concept to which such data are subordinated. It is for this reason that the control exerted over reasoning, in view of the reality to which it refers, cannot pretend to the same exactitude which belongs to the judgment of its formal coherence.

The movement of thought being followed, as we have said, in two opposite directions, leads us, in the last analysis, to distinguish a *real inductive logic,* and a *formal logic* often regarded as *deductive,* which aim at the selfsame purpose of knowledge.

Yet it is important to determine the value of this distinction, in relation to the well-known criterion of deduction and induction.

Deduction proceeding *from the general to the particular* does not characterize strictly formal logic, because the processes of a complete induction also form an integral part of such a logic. If then we would speak of formal logic as "deductive," we should bear in mind that we thus grant to "deduction" a *more extended meaning* than the traditional one, conformably to the use of the word among mathematicians.

The place of deduction and induction in the cognitive process, when the terms are thus understood, seems quite clear if we follow the logical scheme of Jevons. (Chap. II, § 21.) The office of deductive reasoning in this process is to "transform such hypotheses as are inaccessible to actual experience into other *equivalent* hypotheses, which can be verified or disproved by experience."

By these considerations we have brought out more clearly the importance of having an accurate instrument, whose value may be controlled independently of verification, and independently of the truth or falsity of the hypotheses. Such an instrument must be independent of the contents and of the application of the reasoning process.

In the following pages, we have decided to use the name "logic" *in its formal sense,* which corresponds to this accurate instrument of transformation. We shall also have occasion to see the difficulties which arise from extending the use of the word to denote the study of scientific induction, in its broader sense. To avoid confusion, we shall distinguish that study from *pure logic,* by calling it *applied logic.* And in treating of the latter, we shall investigate the actual value of the formal laws of reasoning and the conditions under which they become criteria for the verification of facts.

§ 2. VERBAL FORMS AND SYMBOLS.

But we have really recognized only the utility of that formal logic which frames the laws of thought independently of its content. It remains to be seen in what sense this independence may actually be reached, and in an accurate way. And this investigation is the more necessary, because the possibility of this logical independence of actual content has been contested even by some of the most illustrious disciples of critical philosophy.

We may try in two different ways to demonstrate the possi-

bility of a purely formal logic. The classic method consists of the establishment of *verbal schemes* corresponding to the ideal forms of exact thought.

If we succeed in giving to these schemes a strict and abstract generality our assumption is demonstrated. But in case we do not succeed, it would not be allowable to come to the opposite conclusion, because the defect of the scheme might be caused by the insufficient adaptation of language, which does not always adequately correspond to the processes of exact thought.

Let us consider, for example, Aristotle's rules for the conversion of propositions, concerning which our own Tocco has made some acute observations. A universal affirmative proposition may be converted *per accidens,* according to the phraseology of Boethius, into a particular affirmation. Thus, for example, from the proposition "all squares are parallelograms" we deduce another statement, "some parallelograms are squares." But we then see that the case in which the predicate is essential to the subject is not to be accounted for by this rule. For example, the proposition: "All triangles are polygons the sum of whose angles is equal to two right angles," may be converted into the following: "Some polygons the sum of whose angles is equal to two right angles, are triangles," a statement which admits of the erroneous belief that there may be polygons having *more* than three sides, the sum of whose angles is yet equal to two right angles.

But what does this exception prove? Only this, that the word "some" has an ambiguous meaning. Mathematicians take it always in the affirmative sense, which does not exclude the totality, and they consequently admit that the proposition: "*All* A's are B's" is consistent as a particular case with the other statement: "*Some* A's are B's." But according to the common usage of grammar, there is added to the word under discussion a negative meaning, through which the idea of "some" excludes the idea of "all," and hence the two propositions just noted seem contradictory.

To sum up: The formal rule of conversion to which we have referred as an example holds strictly if we fix, by some fitting convention, the meaning of the word "some," departing from the ordinary grammatical use, and using the standards universally adopted by mathematicians in all analogous cases.

To the example we have chosen we might add many others in which the logical rule as verbally expressed is found to be defective, or to leave room for false applications, through analogous

causes pertaining to the use of language. In particular, the double meaning of words constitutes in itself a common source of many well-known sophisms, such as those whose middle term is understood with a different extension of meaning in the two premises of the same syllogism.

It is well known how much logicians have busied themselves with such fallacies of reasoning, and how zealously they have studied the grammatical part of logic, which aims, so to speak, at the correction of language. Indeed language is always so far from the ideal of the logicians!

Going beyond the need of representing things, and evolving through the impulse of the associations and analogies suggested by reality, language attaches to a quasi invariable sound a meaning which becomes enlarged and transformed and altered in the course of time, so that at every moment there are generally many images, only in part coinciding, but which yet correspond to the same word. And this is, for the rest, an essential condition of progress.

In the face of such facts, grammatical logic pauses, being forced to admit that the verbal schemes of reasoning do not offer a sure guarantee of its exactitude, and in fine do not satisfy the most subtle demands of formal accuracy. From this defect there comes an impulse in a new direction, in which the examination of verbal schemes is replaced by that of *conventional signs*. In this way begins the construction of a *symbolic logic* modeled after algebraic symbolism, and called by the name *logic, pasigraphy* or mathematical ideography.

The first foundations of this construction are due to Leibniz, towards 1700, while Lambert further perfected it about the year 1750. Then these developments were interrupted for about a century, after which they were taken up in a partial and independent way by the English school of geometry (Peacock, De Morgan, Boole), and by H. Grassmann and W. R. Hamilton. The latter should not be confused with the originator of the Quantification of the Predicate, a doctrine which does not go so far in the direction of symbolic logic. Peirce and Schroeder (1877), Peano (1888-89) and Frege (1891) have extended and completed logical symbolism, so as to make it capable of expressing complete mathematical theories.[1]

Whatever may be the value of the system as an instrument

[1] See the *Formulaire de Mathématiques*, published at Turin under the direction of G. Peano, with Padoa, Pieri, Vacca, Vailati etc. as collaborators.

of mathematical criticism, (and we have already pointed out that defect of the purely formal criteria which some scholars seem to find in the studies of geometrical principles) still we must recognize that it forms, in several of its aspects, a great advance upon scholastic logic, and that it responds more adequately to the problem of arranging the schemes of exact reasoning.

To attain this purpose, mathematical logic abandons the use of the customary verbal forms, and creates for itself *ex novo* a dictionary of signs, each of which is made to correspond, without ambiguity, to an object.

But abstracting from the concrete meaning of the objects themselves, this method proposes to represent their logical relations, which a thorough analysis succeeds in settling and in reducing to a few general types, these types also being symbolically represented. The reasoning process therefore finds expression in a species of logical calculus, analogous, as we have noticed, to algebraic calculus.

In particular, the mathematical logicians of the school of Peano have carried this analogy further by working out a deduction of the *theorems of logic,* which contain combinations of a few fundamental principles of reasoning.

In fact symbolic logic, built up as a deductive theory on the model of arithmetic or geometry, gives us ready made, and abridged or verified, certain developments which may be frequently useful in the different sciences.

§ 3. SYMBOLIC LOGIC AND PSYCHOLOGICAL LOGIC.

The second way which serves to establish the possibility of a formal logic is the direct study of the process of thought in the light of its scientific results, and apart from any particular expression by means of words or signs. And just here it will be useful to return to those recent developments of mathematics from which symbolic logic itself—for this reason sometimes called mathematical—has obtained its nourishment.

And so let us contrast with the traditional concept of grammatical logic, or more generally symbolic logic, the concept of a *psychological logic,* which in connection with its schemes and signs does not regard the written formulae so much as the conventions and standards that are not expressed on paper—and which indeed are unintelligible apart from psychological reflection—but which govern the methods of combination.

Logic thus understood, is no longer a deductive theory serving as an auxiliary to scientific developments, but a science of observation and comparison having as its special object a critique of the elementary processes of thought which are shown in the fundamental principles of reasoning. These processes it is that logic attempts to explain as a psychological reality.

Meanwhile we must dispose in advance of a common objection to such a conception of logic, by determining the relations between logic and psychology.

In distinguishing the one from the other, it is generally observed that the former has the *character of a standard for the attainment of truth,* while the latter is simply a description of mental processes, *correct* or *erroneous.*

According to our point of view (the strictly formal), it is important to correct the idea that logical standards have an *a priori* value with respect to truth. But for a discussion of this point, the reader is referred to the second part of this chapter.

Let us admit, in any case, that logic may be regarded as a collection of rules, which *ought* to be observed, *if we desire* to give coherence to thought. But this idea may also be expressed by saying that amongst various mental processes, we can distinguish some in which certain conditions of coherence are voluntarily satisfied, and just these modes of thought are called logical processes. *In this sense logic may be regarded as a part of psychology.*

§ 4. THE POSSIBILITY OF FORMAL LOGIC FOUNDED UPON THE DEVELOPMENT OF MATHEMATICS.

Let us examine from this psychological point of view, the recent developments of mathematics, to which we have already referred. We shall thus be led:

1. To recognize *a posteriori* the possibility of formal logic.

2. To define exactly: "What is logical," in contradistinction to those processes of thought in which the logical development has not become independent of the special modes of presentation that have been used.

The possibility of formal logic results above all from this: The same strictness, the same inflexible coherence, governs the developments of thought in the varied realms of mathematics whether they are close to physical application, or are more distant and abstract, as is the case in the geometry of *spaces of several dimensions* or the *non-Euclidean* or the *non-Archimedean* geometry etc. And

these abstract theories have in themselves an analytical existence, as logical constructions, even if there exists no real object to which they refer.

But this is not all. Thanks to Pluecker, an extremely fruitful idea has been introduced into geometry, which furnishes a principle for the transformation of theories, based precisely upon their formal value.

A geometrical theory may be regarded as a system of logical relations, holding for certain concepts designated by the words "point," "line," etc. We may attribute to these words an abstract and indeterminate meaning, thus regarding them as the symbols of unknown concepts, but such as formally satisfy the fundamental propositions which express geometrical relations. It is then allowable to decide at will, by some convention, the meaning of our symbols, provided this be done in a way that will fulfil the formal conditions already stated. In this way we obtain an infinite number of possible *concrete interpretations* of abstract geometrical theories.

This is not the place to point out the mathematical consequences of this fruitful principle. We will only remark that it has developed in critical minds the appreciation of formal logic and has furnished *a posteriori* the most conclusive proof of its possibility.

Such an appreciation is found, clearly expressed, in the more recent accurate systematizing of geometrical principles by Pasch, Peano, Veronese, Enriques and Hilbert. Pasch seems to have furnished the first model for such systematizing in his *Vorlesungen über neuere Geometrie* (1882).[1]

The critique of geometry already referred to and especially the comparison of its methods result not only in the *demonstration* of the possibility of formal logic, but also in precisely *defining* in what sense we should understand this formal side of logic.

To this end we must recognize (apart from any particular grammatical or symbolic expression) the requisites to be observed in the form of our definitions and postulates, and what we are to accept as a *strictly logical* arrangement of a theory, or on the contrary, what methods of treatment must be regarded as *not rigorous.*

§ 5. A CRITIQUE OF DEFINITION.

The idea of deriving the study of logic from the development of mathematics, and in particular of geometry, is by no means

[1] See F. Enriques's article "Principien der Geometrie" in the *Encyclopädie der mathematischen Wissenschaft,* edited by Teubner, Leipsic.

modern. In fact Aristotle himself took geometry and rhetoric as the basis of his constructions. But the progress of criticism already discussed serves not only to complete Aristotle's rules of *deduction*, but still more to build up a new doctrine of *definition*, which may be considered as quite a recent attainment, to which different thinkers have contributed, in various ways and in part independently of each other during the last twenty years.

The next step in the analysis of the logical process of thought is to make use of this critique of definition. Common opinion attributes two contradictory meanings to definition. On the one hand, it is regarded as a process of reduction, which, certain concepts being given, permits us to form new concepts. On the other hand it is supposed, even if not explicitly stated, that definition can establish the actual meaning of any concept whatever. In practice we bring back the more complex to the simpler notions, further explanation of which seems useless. But if we are asked to define the latter, we shall not undertake to explain them by means of the former.

Thus we plainly become involved in a vicious circle, as will be readily understood by any one who undertakes to make a dictionary of any language.

Now we cannot get rid of this contradiction unless we admit that there is a real difference between some definitions and others, according to whether they treat of a genuine logical process of reduction, or of the formation of those primary and elementary concepts which serve for the construction of other and more complex concepts.

This difference will very plainly appear to any one who undertakes a critical examination of a treatise on geometry.

The treatises for elementary schools explain what the fundamental figures are by showing suitable models of them in the shape of drawings. They thus give a *concrete definition* of the simplest terms of geometrical language.

But this method no longer satisfies the mathematician who wishes to explain the same things to more advanced students. For him the concrete definition of the ideal figures of geometry is only a suggestive means of arousing certain ideas in the mind of the pupil. It has in fact a psychological character which no longer pleases him. And in the full consciousness that there is a sharply marked difference between rational and empirical geometry, and viewing the latter with considerable scorn, the advanced mathema-

tician attempts in his treatise to define everything in a purely logical way.

Take for example Legendre's definition of a straight line, a definition still used in some treatises: "A straight line is the shortest line which can connect two points."

It is interesting to pause for a moment and examine what this definition teaches and what it does not teach. By hypothesis it undertakes to be a logical definition. In this sense it declares that the concept *straight* includes two other more general ideas, that is, the concept of a *line* and that of its *length*. If we should attempt to determine accurately the real meaning of these two notions, we should find ourselves considerably embarrassed. But it is easier not to delay over this question. We will not insist upon the point, but will go on with our study.

We have the right to demand at least that from the definition we should be able to deduce the properties of "straightness," by appealing to certain general properties of *lines* and of their *length*. But even the simplest property, according to which two straight lines can only have one point in common, constitutes a proposition which we may view as indemonstrable by this method,[1] and which we are therefore constrained to add to our definition in the form of a postulate.

In the successive development of more complex figures, we have occasion to recall this postulate or other analogous ones, which bring to light the simplest properties of the straight line. But we get no help from that property which constitutes the so-called definition of a straight line.

Then if we do not want to change the ordinary sense of the word, we must admit that the so-called definition of Legendre is no definition from the logical point of view, or at least that it occupies no such place in that logical system of elementary geometry which we have been discussing.

But if the definition in question does not teach us those fundamental properties of a straight line, which are used in the logical development of geometry, we must not deny that in any case it teaches us something. In fact pupils find the old definition perfectly clear, and willingly admit that it gives them a precise idea of a straight line.

[1] The impossibility of this demonstration is mathematically proven, as soon as the concept of a "line" and of the "length of a line" are defined by suitable postulates corresponding with ordinary intuition.

Now how has this undeniable result been obtained? That is easily understood. Since the general concept of a line calls up the mental image of a thread, speaking of the shortest line between two points, brings before our eyes the picture of a tightly stretched thread, which is a good perceptible representation of a straight line.

But then this so-called definition serves no better for determining the qualities of a straight line than any other similar method of illustration by means of a concrete model. Hence that teacher is mistaken who uses this definition and supposes that it is an essentially different thing from an illustration.

We have chosen as an example Legendre's definition of a straight line, which critical study, to tell the truth, has long since put in its proper place, although it still prevails in some of our schools. We could develop similar reflections with regard to the definitions given by ancient and modern geometers, from Euclid to the present time, omitting only the most recent critical authors.

We ought to distrust those definitions which are to be found at the beginning of works on geometry. They are not definitions in the logical sense of the word, but simply *descriptions* or definitions in the psychological sense.

For the rest, it is easy to see the difference which separates them from the definitions properly so called, which are to be found in the same treatise after the first few pages. These latter may if you choose be avoided, by replacing the definite expression by a circumlocution. But just try to do this in the case of those earlier definitions! The very proposal is immediately seen to be absurd. And indeed, how would it be possible to develop any kind of geometry after we had eliminated one by one from the language, all the words which connote geometrical ideas?

However we try to get around the difficulty we cannot escape this fundamental conclusion which results from the previous observations: There is in geometry a distinct difference between two kinds of definitions, according to whether we are dealing with primary concepts, or with those which arise during the later development of science. Only the latter definitions are so perfect, that it is permissible to replace the term defined by the terms employed to define it. Only these definitions precisely determine what is signified.

Only definitions of this latter sort, then, should be regarded as true logical definitions. And on the other hand, the customary definitions of the earlier geometrical concepts cannot be regarded as true definitions, unless in an extended sense of the word. And

even in this sense they are so to speak *psychological definitions,* a name which shows their office in recalling certain *images,* and in suggesting the notion of their relations.

§ 6. REAL DEFINITIONS AND NOMINAL DEFINITIONS.

The reflections which we have developed by using geometry as a starting point may naturally be extended to any deductive science or theory.

Thus it is easy to perceive in a code the sharply marked difference which separates the definitions of fundamental legal concepts, for example those of *property, contract,* etc., from the definitions of particular possible objects of property, as a *treasure* for example, or of special contracts, as a *guarantee* or a *pledge.*

The former are of so little worth as complete and accurate definitions that we cannot understand them unless we bear in mind the sum total of the successive provisions of the code in question. We may well apply to such definitions the prudent maxim of the Digest (leg. 202, 50, 17) "Omnis definitio in jure civili periculosa est; parum est enim ut non subverti possit."

There are generally found, in every deductive theory, two kinds of definitions. Those which serve to establish fundamental concepts are considered *real,* while those which we introduce as we go are called *nominal* definitions.

Now the only office of these real definitions is to suggest, in a concise formula, some of the known or supposed properties of concepts, to which we must usually add other hypotheses or explicit affirmations. And in this sense we cannot deny the importance of this class of definitions.

But when, in place of attributing to them the value of simple descriptions, we give them the name "definitions," we fall into the danger of fixing our attention upon a formal connection between ideas regarded in some particular aspect, rather than upon their real meaning, and thus we may retain the illusion that a verbal explanation can take the place of a summing up of observations and experiences, having as their object the determination of the meaning in question.

We should say then that *a real definition is not a logical definition,* but only a *psychological definition,* that is, a way of causing certain conceptions to arise in other people's minds, by means of images suitably aroused and associated.

And so the most precise and typical real definition is the *concrete*

definition which we give to the name of an object when we point out the object itself and at the same time pronounce the word that denotes it. When we are trying to define a more general term, but one which still refers to concrete objects, we may point out a certain number of these objects and call attention to their common characteristics.

Although these cases are simpler than those which we often have to consider, the foundation of a concrete definition always consists in establishing an association between a word and a certain series of sensations.

Definitions, considered from this point of view, belong rather to the realm of observation and experience than to that of logic. Thus, for example, it is the office of anatomical and physiological observation, which belong to the realm of zoology, to establish precisely the characteristics of the animals which are to be classed in a certain species.

However, this concrete definition does not serve to establish ideas that are somewhat abstract, and concepts that have been built up by a longer system of associations and abstractions in the process of thought. In these cases the perception of an object does not always suffice to make other people's minds accomplish the same piece of work that has been accomplished in our own. And at this point we usually resort to a description which, by giving an impulse to the mental process, fulfils the office of a psychological definition.

If we can be sure that the formation of the concept is accomplished in other minds in the same manner as in our own, then the original significance of the word by which we represent the concept is established. This is precisely what takes place in elementary geometry, where the sight of some merely approximate model, or any simple description, suffices to recall the process which has already been accomplished in the mind of each person, and thus to awaken a clear idea of the entities (point, line or surface....) which we are trying to define.

But there is usually an uncertainty as to this relation which is reflected in the meaning of those words which connote general or abstract objects. The description then gives us no longer an exact determination, but clears up the real sense of our concepts only in an incomplete way which is hardly accurate.

§ 7. IMPLICIT DEFINITION.

The difficulty which now appears is exceptionally serious, because any indeterminateness in the original meaning of the primary concepts is sure to be reflected in an indeterminateness in the deductive theories relating to them. And the more fully the theories are developed, the greater will this evil become.

A deduction starting from insufficiently established data, even if its hypotheses are of the best, can have only a subjective value, and philosophers ought never to lose sight of this fact....But in science, even though it may have a hypothetical and analytical character, this lack of determinateness in the fundamental data cannot be tolerated, or at least not beyond a certain point, which gives the *practicable* degree of *accuracy,* and which must be prudently settled, in relation to the errors of observation and experiment.

But, logically speaking, a deductive theory cannot be called satisfactory, unless all indeterminateness is rigidly eliminated from its data. This perfection is, in any case theoretically, attainable *at least formally,* when leaving the first concepts explicitly undefined, we give an *implicit logical definition* of them by means of a system of postulates. And this is exactly the way that geometers have taken in order to give geometry a logical structure.

Thus, the statement of a deductive theory will include certain symbols A, B, C .... representing undefined fundamental concepts, and a certain number of *logical relations* among these, assumed as *postulates* or primitive propositions, from which all the other propositions are to be deduced as *theorems.* The *sum total of the postulates is to be regarded as forming the implicit definition of the given concepts,* in so far as is needful for the theory founded upon them.

When we say that: "The postulates represent the logical relations of the fundamental concepts A, B, C ...." we mean that they take on a general or abstract form, such that they remain intelligible even when cut off from all mental images of these concepts, so that we retain only the fact that A, B, C .... are obtained, for example, by the synthesis of certain undefined elements etc.

Not all the observable relations between A, B, C .... can be reduced to this logical form, unless we permit the *addition* to the data of *new concepts* which are *not defined.*

The analysis of the thinking process will hereafter lead us to a psychological explanation of these logical relations, as the expressions of supposed associations and dissociations of objects. Mean-

while we will give some examples which may throw some light on the distinctions already made.

The postulates:

1. Two points belong to one straight line and to one only;

2. A straight line and a given point outside it determine a plane;

3. A plane contains the straight line to which any two of its points belong;—

these postulates, I say, express the logical relations between points, straight lines, and planes conceived as "classes of points."

On the other hand those propositions in which we speak of the *motion* of figures—made up of points, straight lines and planes—or of their congruence, or equivalence, do not express logical relations existing among these concepts, because one can understand them only by observing the figures themselves. And we are only able to abstract from this perception after we have added to those undefined objects a new concept resulting from the analysis of some imaginary experiments on motion, as for example the concept of a certain group of correspondences among points etc.[1]

It is important to understand clearly what is the value for knowledge of this implicit definition. When for example in a discussion of geometry, the point, the straight line etc., are defined by means of postulates, we have what we need in order to decide whether certain logical relations among these concepts do or do not exist. But whether the object designated by the name "point" is something approaching a physical corpuscle, or instead is any other object you please, for example, a circle conceived as an element in the system of circles on a plane, there indeed we have a question which our postulates do not in any way enable us to solve, because diverse objects, in case they form a class endowed with certain abstract properties, may fall equally well under the same geometrical treatment.

In other words, the *implicit definition* does not serve the same purpose in a theory as the *real definition* which determines the fundamental concepts or the primitive symbols; but it serves rather as a *substitute* for the real definition *in relation* to the formal judgment concerning the deductions of the theory itself. In fine, the implicit definition allows the possibility of an infinite number of different determinations of the real signification of the symbols; although

[1] Cf. the collectanea of F. Enriques, *Questioni riguardanti la geometria elementare* ("Questions Concerning Elementary Geometry"), Bologna, Zanichelli, 1900, passim and in particular the monograph by A. Guarducci.

they are obliged, as we have said, to verify formally the fundamental propositions which have been postulated.

However, in the actual developments of science, we need, from the standpoint of reality, to fill out the implicit definition by means of a *concrete interpretation*. Thus we shall fix by means of suitable observations and experiments, the sense of the undefined terms by which we try to represent concrete objects or their relations. And so we shall determing what facts, or supposed facts, we mean to express in the postulates, in order that the real content of those concepts which remain undefined may be limited by the relations thus established.

Let us observe that its incompleteness from the point of view of reality is no *defect* in the implicit logical definition, when we consider the scientific office of the deductive theory based upon it. In fact we are undertaking an empirical test of a certain system of hypotheses by first finding their logical consequences. To this end it is *important that these hypotheses shall be taken in their entirety and isolated from all other hypotheses* which the facts might suggest. Evidently we can attain this end in a more rigorous way if we employ a method of drawing conclusions which provisionally neglects the facts themselves.

§. 8. EXAMPLES: CONCEPTS FOUNDED UPON PHYSICAL DATA.

To illustrate what has already been said, let us show by means of some examples the critical procedure which should be employed in order to obtain the implicit definition of a concept or of a system of concepts.

First of all we should distinguish *two* different ways according to which we may suppose the *concept* given:

1. Either we are dealing with concepts that we desire to have determined in such a way as to represent a certain physical reality;

2. Or we are dealing with concepts, however they have been acquired, which we consider in their psychological reality, without concerning ourselves as to whether or no they correspond to some external object which they are supposed to represent.

Our first case is taken from the theories of mathematical physics, whose concepts are implicitly defined as soon as we have established equations between certain quantities, furnished by suitable processes of measurement. Here these quantities are our undefined objects, and the equations are the postulates which express their logical relations. The measurement is the means of concrete interpretation,

which serves to give to the deductive theory its proper physical signification.

Now the development of this theory by the aid of experiment comes to modify our equations, and hence to *correct* progressively the implicit definition, which therefore *expresses at every moment of the progress of the theory, the most advanced synthesis of the data thus far obtained.*

The development of a theory, and the comparison of the measurements furnished by the fundamental quantities, sometimes result in indirectly modifying the equations, by modifying the concrete interpretation of the quantities which they connect.

A simple case in point is the evolution of the concept of *temperature,* which may be schematically described as a succession of three definitions, relating to knowledge experimentally gained, and growing in precision:

1. The recognition of the fact that the sensations of heat are connected with the expansion of bodies, and the resulting definition of the increase of temperature as proportional to that of volume.

2. The recognition of the fact that the expansions of bodies at *equal temperatures* are not always proportional, and hence the conclusion that the measure of the temperature is *relative* to the *thermometer.* Hence the selection of *gas thermometers* (because of their agreement amongst themselves) as means of defining "temperature."

3. The recognition of the disagreement between gas thermometers, as measurement grows more precise; and the resulting construction of an ideal standard of measure, which corresponds to the totality of the thermometers, that is to say the *absolute* temperature. (Cf. Chap. VI.)

§ 9. CONCEPTS FOUNDED UPON PSYCHOLOGICAL DATA.

When we are expounding or reconstructing concepts which have grown up during the development of thought, and which are to be regarded simply as the products of that thought, then the process of forming by analysis an implicit definition appears in a different light. For we are then entering upon the realm of psychological and *historical criticism.*

With this kind of criticism we may class in some respects that analysis which aims to determine the precise meaning of a legal concept in a system of positive law.

If, for example, we have the problem of explaining in this

sense the concept of property according to the laws now in force, our critique may take up the question from two points of view:

1. Either we may seek a simple interpretation of the express purpose of the legislator;

2. Or we may try to find out what concept of the legal institution in question results from the synthesis of all the actual conditions implicitly included in a law.

In the first case our effort is merely to define a concept founded upon psychological data. In the second case the problem partakes both of the character of a psychological study, and of that of a critique of reality, inasmuch as certain actual social relations are compared with the ideas of them as embodied in laws and in jurisprudence.

If we stop a moment to think about the problem thus indicated, even in its more restricted sense, we shall quickly perceive the difficulty peculiar to such a class of questions.

First of all it is clear that article 436 of the Civil Code[1] cannot possibly be regarded as a definition of property, whether because it reduces the concept to the general one of "rights," or because, describing only a complete and unmodified ownership of property, it is insufficient to establish the limits and diminutions which property rights must undergo in various cases.

For the rest, as we have already pointed out, a truly logical definition of property cannot even be demanded, because it would necessarily be founded upon concepts equivalent to those which we are seeking to explain. We might better undertake to determine by means of suitable statements, "what are the modes of dealing with things which the law allows to the proprietor and forbids to other persons."

Precisely these points ought to be determined, by taking account:

1. Of the physical nature of those things to which the term "property" is applied—as movable and immovable things, or immaterial things such as credit, the rights of authorship, writs etc;

2. Of the legal status of these same things, that is to say of the actual property rights connected with them;

3. Of the relation between the proprietor and the thing (whether he has it in his possession etc.).

[1] "Property is the right to enjoy and to dispose of things in the most absolute manner, provided that one does not make use of them in a way that violates the laws and their provisions."

§ 10. THE LOGICAL PROCESS: THE STATEMENT OF PROBLEMS.

In the developments already outlined, after having characterized formal logic in a psychological sense, we deduced its possibility from the development of mathematics. From that point we were led to begin the examination of definition, and in particular to recognize the implicit definition of concepts as logically fundamental. Certain questions relating to the manner of forming implicit definitions have occupied us during the last two sections, whose contents scarcely belong in the realm of pure logic.

In whatever way scientific criticism has succeeded in implicitly defining the concepts of a deductive theory, its logical development requires only that we should suppose to be given:

1. Objects,

2. Logical relations between these objects, subordinated to certain conditions, which we ought then to examine.

And the development consists of:

1. Nominal definitions forming new objects;

2. Deductions—in the wider meaning of the word—which add new logical relations to those already existing between the objects given and constructed.

Every one admits the importance of deduction, but the office of the explicit logical definition is not so well recognized. The very designation "nominal," makes us think at once of the simple introduction of a name, and so of an economy of words on the part of the author. It is well, however, to point out that the name, or the abbreviated form of speech, *sets its seal* upon a completed constructive process of thought, and this process it is that gives its proper value to the definition.

Thus, in a treatise on geometry, the words "polygon," "polyhedron," "circle," "sphere," could be replaced by longer expressions, in which we should speak only of the primitive elements, point, segment, plane, distance, which enter into the figures named. But we should then still insist upon certain groups of words or signs, corresponding to certain ways in which the elements are associated. And the purpose of considering the groups that are formed during the development of science, is equivalent to the definition under discussion.

We can find similar examples in physics, in which certain concepts, such as that of the "potential function," being nominally de-

finable, by means of other concepts already given, form a most important acquisition.

The definitions and deductions which form the development of every theory, are to be regarded, from our point of view, as *psychological operations,* while the totality of these will be called a "logical process."

Thus there now arises the problem of *explaining* the logical process *psychologically.* For this purpose let us first examine the *fundamental logical operations,* which correspond to the definitions, pausing a moment to show how these operations permit the formation of *purely logical concepts,* and how their classification is reflected in an analysis of the elementary types of definition. By the aid of such operations, we shall then see that there *can be constructed* not only concepts but certain *logical relations* between them. And we shall thus be led to ask whether those *logical relations* which we regard as *given* (for instance the postulates of a theory) may be held to be constructed by an analogous proceeding. This question will be answered in the affirmative, provided that we enlarge sufficiently the signification of this process, in relation to the objects of thought that we assume.

Finally we shall examine the conditions of the possibility of the process to which we have referred. That is, we shall treat of the *logical principles,* either with respect to objects taken as the elements or units of thought, or with respect to the conceptual constructions themselves. And we shall also consider those questions about the *compatibility* of the postulates which hereupon arise.

§ 11. LOGICAL OPERATIONS.

Let us suppose given objects of thought, which we may regard as elements or units, capable of entering into certain combinations. Let us suppose that these elements satisfy certain *conditions of invariability* which we shall later find expressed by logical principles.

The psychological *associations* and *dissociations* which fall within the realm of clear consciousness and volition, constitute the *fundamental operations of logic,* and enable us to form new objects of thought, by starting from those already given.

We can associate several objects by conceiving them as *successive* or *simultaneous,* and thus we come to

arrange them in a *series* (an ordered group),

or else to

reunite or *conjoin* them in a class (a group, an aggregate etc.)

Under certain conditions we can arrange several series in one *composite series,* or series of series. And we can always bring together several classes of any kind in one composite class (class of classes). By arranging two classes or by putting two series together, we may succeed in relating the elements of one class to those of another, according to a correspondence etc.

Besides the *inversion* of the order of a series or of a correspondence, we may consider, as contrasted with the processes already discussed, those which are essentially dissociative, or *disjunctive.* And these lead us to interrelate the component classes in a composite class, that is to determine, where it exists, the totality of their common elements overlapping. Or again we can *abstract* from the distinction of the elements of a class, that is construct an object, "the abstract concept of the element of a class," which may be represented by any element of that class, which we think *capable of being substituted* for, or equivalent to any one of the other elements.

§ 12. PURELY LOGICAL CONCEPTS.

Logical operations lead to the construction of new objects of thought, on the basis of the data.

If we consider all the associations in classes that have been made, and then survey the classes thus obtained, we may rise by a further abstraction to the general concept of classes or *aggregates.*

In a similar way we form the abstract concepts of *series,* of *correspondence,* of the *overlapping of classes* etc. Since these are constructed by the use of purely logical activity, applied to any objects whatever, they may be called *purely logical concepts.*

We should expressly note that the constructive operations which form these concepts involve no transcendental procedure, provided that we are referring to objects actually conceived by the mind, of which there are only a finite number. Only in relation to this hypothesis will there be the opportunity later to extend the significance of this sort of concepts.

§ 13. ELEMENTARY TYPES OF DEFINITION.

Let us stop a moment to see how this classification of logical operations gives us a means of distinguishing the elementary types of definition in terms of purely logical concepts.

We will take up, for special examination, three fundamental

types: definition by the conjunction of elements, definition by overlapping, and definition by abstraction.

A definition of the first type is one in which we present the object as the *totality* of several given elements, or as a *series* made up of several objects arranged in order.

For example, the *perimeter* of a polygon is "the totality of its sides." A crowd is "the aggregate of the men who are gathered together in a certain place" etc. A *row* of trees, a *succession* of events, are series composed of elements taken in a certain order.

The definition by the common portion or overlapping is substantially the scholastic type of definition, which is thus described: "Definitio fit per genus proximum et differentiam specificam." This definition consists of determining a class of objects as common to two or more given classes.

Let us take for an example from Fiorentino the definition of inheritance given by Cicero in his "Topica": "Inheritance is a patrimony obtained by the death of some person, by virtue of a *right.*" We here regard as given two classes of patrimony, those which are ours by rights of any sort, and those in some way obtained by the death of a person. Inheritance represents the logical units common to these two classes.

Here are some other examples: In zoology, the definition of the various species of animals consists of determining each of them as the "overlapping" of larger classes—existent or conceivable—possessing certain characteristics. In geometry, the surface of a triangle is defined as the portion common to the half planes bounded by the sides and containing the opposite vertices etc.

The definition by abstraction is more important and less well known. Grassmann, Helmholtz, Mach, Maxwell, and in Italy Peano and Vailati have shown the use of this definition. Let us examine its foundation: Given a class of objects, which (under that particular aspect in which we find them associated) are assumed to be *equal,* by this very fact without further explanation, the abstract concept of an individual belonging to a class is defined.

This is substantially the implicit process, according to which in language all abstract terms are defined.

Euclid's definition of the ratio of two quantities is the oldest known example of a definition of this type. It consists simply of stating in what cases two pairs of quantities should be regarded as having equal ratios, that is, as being proportional. We may say then: "The ratio of two (homogeneous) quantities is the abstract

concept of their union in a pair, in so far as one considers this pair as equal to any other pair of proportional quantities."

Given a class of parallel straight lines, by regarding them as equal, that is as capable of being substituted one for another (with respect to direction), we thus have defined the abstract concept of their *direction*. Thus we may say: Direction is the abstract concept of a straight line, in so far as we conceive that any one of its parallels could be substituted for it.

In economics, the concept of *value* is introduced by an analogous process. Given a class of goods exchangeable in a certain market, and so regarded as equal, their value is defined by abstraction. And so the value of goods (in a market) is the abstract concept of the goods, in so far as we consider them equal to any goods for which we can exchange them.

We should add that the definition by abstraction supposes facts, as we easily see from our last example. In fact if we wish to determine the value of merchandise, we must limit ourselves to a market in which, for a certain length of time, the same goods maintain amongst themselves the same rate of exchange.

Nevertheless this remark does not limit the possibility of definition by abstraction in so far as we view things from the logical side, and conceive our classes of elements as fixed. For then we can regard definitions by abstraction, and also by association, as operations that are always possible. But this is no longer the case for definitions by overlapping. Their possibility depends upon the condition that the overlapping classes shall have elements in common.

§ 14. SECONDARY LOGICAL RELATIONS AND AXIOMS.

Whenever several given objects are in different ways united in classes, or arranged in series, etc., we imagine between the objects themselves, or the classes or series that have been arranged, certain *logical relations* which express the operations performed. For instance, if we, with the three objects *a, b, c,* construct the class:

$$D = (abc),$$

we can say that

a belongs to D,

or that

a is in D, etc.,

and the relation thus expressed signifies that D is a class formed by uniting *a* with other elements.

So also between the classes

$$A \equiv (bcd)\ B \equiv (acd),$$

we conceive of a relation which is expressed by saying that the classes A and B have a common element.

Speaking in general terms we may say: *The logical operations when performed find their actual expression in certain relations which we conceive as existing between the objects given and those constructed.*

When we consider the totality of the elements, and of the classes, series etc. which are made up of them, the logical relations always appear to have the same character of invariability which we have already noticed in the objects of thought. For this reason the relations already discussed may be combined by means of the fundamental operations. Thus the union of several relations in a system, and the overlapping of several systems of relations, represent in their turn *new relations* between the given and the constructed objects.

It will now be well to define the *equivalence* or equality of two relations or systems of relations. Two systems are equivalent if the same conditions govern the operative processes present in them. Thus for example, in the two systems:

a belongs to D, b belongs to D;
the class (ab) belongs to D;

we have statements which equally imply that the class D has been made up by uniting a, b to some other (possible) objects.

To operate upon systems of logical relations by connecting them, by superposing or overlapping them, and by substituting them for equivalent systems, signifies the process of *deduction.*

Our judgment as to the equivalence of logical relations, and hence the *rules of deduction,* rests upon the recognition of certain *fundamental laws of operations,* which are due to certain relations of purely logical concepts (§ 12) and which are called axioms.

We will give some examples:

1. The associative property of classes expresses the fundamental law of the process of conjunction. In whatever order we conjoin a given totality of component classes, the same composite class results.

2. The *transitive character of equality* (according to the nomenclature of the mathematical logicians) expresses the fundamental law of abstraction, that is to say the "possibility of substituting one

element for another in a class, so far as concerns the abstract idea of that class."

It is important to notice that this axiom which is generally expressed by saying that "two things equal to a third are equal to each other" should not be confounded with the principle of identity. (§ 17.)

3. The *commutative character* of grouping expresses a fundamental law about the relation between conjunction, serial order, and abstraction. That is to say: "If several objects are arranged in ordered series in various possible ways, the abstract concept of the class composed of these series includes the totality of the given objects," etc.

§ 15. PROPOSITIONS.

The commonest form of logical relations, which grammatical logic regards as typical, is the *proposition*. Therefore it is worth while to pause a moment and examine the operative process thus expressed.

By the aid of a *singular proposition* "*a* is *b*" in which the subject *a* denotes a particular individual, we can express the fact that *b* is the *abstract* concept of a class obtained by *conjoining a* with other individuals. For example, "Dante is a poet" signifies that poet is the abstract name of the individuals of a class which includes Dante.

In an analogous way, it is clear that the conjunction of the elements of a class (*a*) with other elements, in a larger class (*b*), from which arises the abstract concept *b* may be expressed by the *universal proposition*

"all *a* is *b*."

On the contrary by means of the *particular proposition*

"some *a* is *b*,"

whose subject is indeterminate, we express the fact that two classes (*a*) and (*b*) have common elements. For example, the proposition "some mammals are bipeds" signifies that the class of mammals and the class of bipeds overlap.

§ 16. GIVEN LOGICAL RELATIONS.

Let us try to recapitulate the results of our critique. If we consider any objects of thought, as *a, b, c, d,* and perform some operation upon them (conjoining them or the like), the operative process is expressed in a system of relations amongst the classes, series, etc., or in general amongst the objects or the concepts that

have been made up from them. And these relations are usually expressed by means of propositions. This system of relations, taken as a whole, helps us to recognize the operations that have been performed; but if we take only certain of its relations, these *partially* express the conditions of the operative process.

But now, side by side with these constructed logical relations, we may think of the *logical relations as given,* such as hypotheses or transformations of hypotheses. These may appear in a completely analogous form, as for instance, in the form of propositions. Hereupon there arises a question which is the *inverse* of that just treated.

This inverse question is: "Whether in any case the given logical relations amongst the objects of thought, as for example amongst concepts, can be interpreted as the conditions of an associative process, such that this process itself, starting with certain suitably determined elements, can generate new objects of thought."

If we look more closely into this matter, we soon become involved in a fundamental difficulty. If in every system of concepts, we try to interpret its logical relations as the conditions of an associative process which shall start from given elements, it sometimes happens that *the number of these resulting elements must be supposed to be infinite.*

We find a simple example in the consideration of the first postulate of the "straight line" and the "plane," by which we attempt to define these entities as "classes of points" so constructed as to fulfil certain conditions.[1]

Now how can we admit an *infinite aggregate of objects* as *data for thought?*

Since the only sense attributable to a *given object* is that it has been actually *conceived,* it is clear that we cannot admit that an infinity of objects are *given,* without a mental process of a transcendental nature.

The matter appears in a different light, if side by side with the objects actually *conceived,* we think of objects as *conceivable,* not really regarding these as *given* but as *supposed.*

If we have a certain system of concepts, amongst which certain logical relations exist, supposing that these concepts themselves may be constructed by means of logical operations based upon certain elements, these concepts then appear in the form of series, classes, correspondences, etc.

[1] Cf. Enriques-Fano, *Rendi conti del Circolo Matematico di Palermo,* t. IX (1894).

If the elements of our concepts have once been mentally pictured in the beginning, the logical relations of these concepts themselves teach us, as we said before, *partially* to reconstruct the process by which they were originally formed. But if we drop the hypothesis of an original state of mind by which these elements have been conceived, the supposed associative process of the concepts has still a significance. The series or the classes—which may now contain even an infinite number of elements—take on the value of *modes of determining a priori the order or the connection of the supposed objects of thought,* so soon as these shall be actually conceived. Take, for instance, the concept of a directed line L as a series of points. This gives us a method of arranging the points which are conceived as being on this line. Hence if we think of three points A, B, C, upon L, we thus have determined a way of arranging points in series, which allows us to choose *one* of the six possible combinations ABC, ACB, BAC, BCA, CAB, CBA, as the order in which the three elements shall be placed within the supposed series L.

§ 17. CONDITIONS WHICH MAKE CONCEPTS POSSIBLE: LOGICAL PRINCIPLES.

What we have said about the interpretation of the logical relations of concepts, and about the definition of objects of thought by means of these relations, holds good, subject to certain conditions of the *possibility of concepts,* conditions already implicitly admitted in the course of our discussion.

It is important to bring out, in this connection, the psychological basis of this possibility. And for this purpose we will begin by recalling the observation that the objects used as the elements of a logical process, are *invariants* with respect to the movements of thought, and especially with respect to the operations of association and dissociation.

The conditions of invariability are expressed by *logical principles* relating to the three *fundamental intuitions* of *earlier* and *later* (temporal order), of *identity,* and of *distinction* which are found as the substratum of every thought.

There are the three following well-known principles:

1. The principle of *identity,*
2. the principle of *contradiction,*
3. the principle of the *excluded middle.*

The first principle states that the object can be recognized as one identical thing through successive representations. The second principle excludes the possibility that two objects which are distinct in one representation, shall be conceived as identical in another representation. And finally the third principle affirms that concerning any two objects conceived at the same time, the judgment of their identity or of their distinction is always possible.

In their totality these principles confer upon the objects of thought a psychological reality, independent of time, and thus form the presupposition of a symbolic logic. The latter aims to depict the genetic process of logical operations, as if it were a system of logical relations present all at once.

For this very reason it is not possible to represent these principles themselves symbolically. For they are not fundamental propositions, used by the thinker side by side with the postulates of various different theories. The three principles are rather, as we have already said, the statement of the requisites to which the objects built up by the logical process must conform. The symbolic expression $a = a$ does not at all express the principle of identity, but rather a mere senseless tautology.

The conditions of invariability, as expressed by the principles of logic, are *voluntarily fixed* for every object which is conceived as such. "To conceive an object" means precisely to determine and to distinguish it as recognizable, that is, to inhibit the course of those unconscious associations, which might tend to modify the representation.

The fact that objects once identified by thought remain invariable in relation to logical operations, is implied in the laws of these operations as formulated in the axioms. The logical relations that have been constructed, signify in the last analysis, that *determinate operations lead to the recognition of a certain identity or of certain distinctions.*

The fact just indicated may be expressed by saying that: "Logical relations built up by the aid of objects really conceived are *a priori compatible*," or that: "Constructive operations are *possible without* ever leading to a *contradiction.*"

Now, if we are dealing with objects, not actually conceived, but supposed as the elements of a concept, the will which posits this concept as given, subordinates and limits, in various ways, the further determinations which are to distinguish or to identify those objects which have not yet appeared in the field of thought. And the *com-*

patibility of these limitations, which result from the definition of the concept, or again the consistency of this definition,—is a necessary condition of the possibility of the process by which these relations themselves are generated.

What is presupposed is therefore this: "The deductive development of the given relations will never lead to a contradiction." For a contradiction would show that some one of the objects required by the definition has not the requisite fixed relation to the associative process in question; and hence it is impossible to conceive this object as falling under the definition in question.

To sum up:

The logical principles express, not only the primary requisites of the invariability of the objects conceived, with respect to the associative operations, but also the conditions for the possibility of concepts. That is, they express the possibility of representing to ourselves the given logical relations amongst the concepts, as constructed upon certain supposed elements used as a basis.

§ 18. THE COMPATIBILITY OF THE POSTULATES OF A THEORY.

How can we ascertain that the given logical relations which define a concept or a system of concepts are compatible? This problem faces us at the outset of every deductive theory founded upon a system of postulates. And this difficulty especially belongs to those cases in which we cannot represent the concepts as made up of a finite number of elements.

The desired judgment may have its foundation:

1. In experience,
   a. physical,
   b. psychological;
2. In the *intuition* or mental image of the concepts;
3. In a *logical demonstration.*

Let us examine the meaning and the value of these three kinds of proof.

1*a.* Although a possible concept need not necessarily represent a real thing, we admit on the other hand the statement that "the conception of that which is real is possible." And we say in precisely this sense that "reality cannot be contradictory." Upon the basis of this principle, we seek to justify the compatibility of a system of (hypothetical) relations, by showing by means of experience, that they correspond to a reality, according to some conventional *interpretation* arbitrarily given to the concepts.

The value of the proof will lie in the fact that the real invariants (Chap. II) can be represented as the objects of thought.

But some remarks are here in order. In the first place the invariability of real things is only approximate, and so the formation of the objects of thought corresponding to it is not well determined. Therefore we need to distinguish at least between *qualitative* and *quantitative* experiences (cf. the second part of this chapter).

Furthermore these actually practicable experiences, which are finite in number, do not suffice by themselves as a proof of reality. An interpretation of these experiences is needed, as we have seen, and this depends upon a conceptual mode of representation. From this point of view the search for an experimental (physical) proof of logical compatibility appears to be a vicious circle, because compatibility is generally a condition premised before verification. In any case the fact remains that the lack of compatibility will prevent verification if pursued beyond a certain point. Furthermore the attempt to test physically the compatibility of certain relations, leads to their deductive development, and hence to a control of the proof 1*b*.

1*b*. This proof consists precisely of recognizing that a deductive development *sufficiently* carried out in diverse ways has not led to contradictions.

Notwithstanding the errors to which the application of this criterion is subject, it has the value of considerable probability in case it is used together with a widely comparative criticism, always taking account of the teachings to be gained from the history of science.

2. The appeal to mental imagery, even though it meets with a difficulty of interpretation which may lead imperfectly critical minds into error, still guarantees the possibility of concepts, inasmuch as it implies a whole formative process of associations and abstractions. These tend to eliminate contradictions from the hypotheses that have been worked out. In this sense intuition represents a wide psychological experience.

3. Logical demonstration constitutes the only *rigidly certain* method of proof, but yet it has only a *relative* value. What one learns from this method is to recognize that, certain concepts being given, we can logically construct others from them, and that from the supposed compatibility of the relations which define the former there also results the compatibility of the relations of the latter.

Let us compare these views with the historical development of

the problems which have suggested them, and of the judgments relating to them. The question of the compatibility of a system of postulates made its appearance in geometry in consequence of the contruction of the non-Euclidean theories (cf. Chap. IV). The founders of these theories first became convinced of the logical compatibility of their hypotheses, from the fact that they met with no contradictions in a relatively extended development of their theories.

But the question was not finally settled until it was successfully demonstrated that the logical compatibility of the non-Euclidean hypotheses is a consequence of the compatibility of the fundamental properties of whole numbers, and of arithmetical operations; or until this compatibility of non-Euclidean geometry was seen to follow from that of the postulates of Euclidean geometry.

In the more recent developments concerning the principles of geometry, the two points of view just mentioned are found to be of equal use. The German school especially, from Riemann onward, have worked in connection with arithmetic, while the Italian school of geometry, ever since the "Essay" of Beltrami, have treated the questions as to the compatibility of postulates by making use of various concrete interpretations, and especially of those suggested by the Euclidean intuition of ordinary space.

It is clear that psychological experience—either consciously carried out or contained in the mental images corresponding to our concepts—equally guarantees that arithmetic and Euclidean geometry are free from contradictions. Furthermore the logical possibility of the one can be reduced to that of the other by means of the introduction of coordinates. Consequently from this point of view the proofs furnished appear as equivalent. On the other hand, on the physical side, the experiments that verify arithmetical relations have an advantage over those belonging to geometry, in that they are qualitative, that is, they deal with relations that vary only in discrete fashion.

But there is another point of view, from which the supposition of the arithmetical compatibility appears as the most limited postulate among all those which can be assumed for the establishment of the possibility of concepts.

§ 19. THE FUNDAMENTAL PRINCIPLES OF ARITHMETIC.

We propose to discuss here quite briefly, some questions as to the foundations upon which arithmetic has been built up, and as to

their logical possibility. We shall appeal in case of need to the critical researches of Dedekind, Peano etc., and especially to those of Peano, which seem to us the most perfect in their logical and symbolic aspect, but which resolutely disregard problems of a psychological or philosophical class.

Numeration can be based on a *physical* postulate. We may take as *given* several *series* of objects

$$a, b, c \ldots$$
$$m, n, p \ldots .$$
$$\ldots\ldots\ldots\ldots\ldots$$

The general terms for a member of the first series shall be A, for the second series shall be M etc.

Let us suppose for each series the following laws:

1. Each object has a determinate next successor;

2. Each object, except the first one, which does not follow any others, has a determinate next predecessor;

3. The following property holds good (principle of mathematical induction):[1] If a class of objects is such that whatever A it contains, it also contains the next successor of A, and if that class contains a, it will contain all the members of the series $a, b, c \ldots .$ (and analogously for the other series).

We can form the following series:

$$a \quad (ab) \quad (abc) \ldots .$$
$$m \quad (mn) \quad (mnp) \ldots .$$
$$\ldots \quad \ldots . \quad \ldots\ldots$$

Gathering into one composite class the objects or classes which are found in the same column of the above array, let us successivly define by abstraction the numbers

$$1, 2, 3, \ldots .$$

The development of arithmetical operations may then be founded upon the logical axioms and the postulates 1, 2, 3. The office of these postulates and especially of the third, (which Peano was the first to consider a defining condition of the system of numbers), consists in making possible *a priori* and without contradiction, the

[1] We should not confound *mathematical induction*, namely the argument from n to $n+1 \ldots .$, with the *complete induction* of Aristotle which was discussed in § 1, and by which we can infer from the abstract concept of a class what is supposed to be known concerning each of the objects that compose the class. Mathematical induction was discovered much later. Vailati informs me that it dates back to Maurolico (1550).

deductions obtained on the basis of the infinite elements and relations which are assumed in the general concept of whole numbers.

Now it is especially important for our purpose to notice, that a series such as $a, b, c \ldots$ can be assumed apart from physical experience, by means of certain *general conventions* which involve a succession of acts of thought that can be carried on without limit.

For example, we suppose two objects a, a, to be given, and we construct the new object

$$b \equiv (aa),$$

obtained by putting them together; we can construct in an analogous way

$$c \equiv (ba), \; d \equiv (ca), \ldots.$$

The series

$$a, b, c \ldots,$$

thus psychologically defined, satisfies the conditions 1, 2, 3.

Also on the basis of the reflexive property of thought we can construct an analogous series:

$$a, b \equiv \text{thought of } a, \; c \equiv \text{thought of } b \text{ etc.}$$

Let us note that the first of these conventional constructions leads to a *dyadic system* of numeration, in which to be sure the *writing* of the figures in order requires a physical presupposition of a geometrical sort.

From what has been said it may be inferred that the fundamental postulate of arithmetic may be founded upon a psychological reality instead of upon a physical reality, that is upon the fact that certain acts of thought may be repeated without limit by subordinating them to general conventions, in such a way as to construct series which satisfy the conditions 1, 2, 3.

In fact arithmetic, needing only to adjoin a psychological postulate to the logical laws or axioms, may be in a sense regarded as a part, or a continuation of pure logic, in which the axioms are filled out by the *principle of mathematical induction* understood as a *fundamental* property of the psychologically constructed series.[1]

[1] The question of the compatibility of the arithmetical postulates has been placed upon the order of the day by the communications of D. Hilbert to the recent mathematical congresses (Paris 1900; Heidelberg 1904). Hilbert seeks for a logical demonstration, but we do not quite see in what sense the views of the illustrious geometer should be understood.

We should also mention an article of a polemic nature by A. Padoa which maintains the empirical point of view, but without distinguishing psychological and physical experiences. The observations made in our last section are partly opposed to the views of this author.

B. THE APPLICATION OF LOGIC.

§ 20. FUNDAMENTAL PROBLEMS.

After having examined the problems that refer to the logical aspect of the cognitive process, in its most perfect subjective expression, let us now consider the same process in its relation to reality. From the study of pure logic we will thus pass to its application. We have already indicated the office of deductive reasoning in the acquisition of knowledge, as an *instrument for the transformation of hypotheses.* It will now be well to examine this transformation more closely and to try to clear up two problems, one of which is subordinate to the other, and which concern the value of scientific theories.

1. When certain hypotheses regarding a class of facts, are transformed by a reasoning process, we assume as *evident* that the reality or unreality of the premises implies that of the consequences. In the same manner we assume the inverse supposition, if we are dealing with invertible deductions, that is, if the given hypotheses and those which have been transformed are *logically equivalent.*

Such a supposition, if we adopt it without restriction, attributes a real value to deduction, and implies that its unconditioned use is legitimate in every branch of science. But we need to investigate the justice of this judgment. For this purpose let us state the general problem: "How is it possible for a logical process to give us a representation of reality?" That is, under what conditions and how does it happen that the transformation of concepts (determined by subjective laws) is reflected in a transformation of real relations, thus implying a bond of coexistence among the latter.

2. The second problem which is subordinate to the first, relates to the method of verifying a given system of hypotheses, when conjcined in the form of a theory. We are dealing then with an attempt to answer the question: "Whether, and within what limits, a given deductive theory is capable of representing a certain class of real relations."

§ 21. LOGICAL REPRESENTATION AND THE POSTULATE OF KNOWLEDGE.

We will now discuss the first problem. The possibility of logically representing reality should be understood in the following

sense. To the invariants of experience, which constitute real things, (objects and relations) we can make the invariants or objects of thought correspond in such a way that to the coexistence and invariable successions of the former, the classes and series of the latter correspond.

The postulate of knowledge stated in Chap. II already approximately affirms this possibility in a general form. But it is clear that if we assume that the logical representation of reality can be carried on without limit, we thus come to take our postulate in a very strict sense, which is more than is required as a foundation for the knowledge needed in ordinary life.

Let us suppose for a moment that we can accept this strict interpretation, that is that we can grant the existence of a logically arranged experience, carried out to an absolute degree of perfection, and then let us ask what conditions are in this way presupposed. We are then led, with Kant, to take the fundamental intuitions of *substance* and of *cause* as *necessary and universal relations,* containing exactly the *a priori conditions for the possibility of experience.* To these conditions are related *synthetic judgments,* equally *a priori.* These judgments would have objective reality because of this possibility.

But the conclusions drawn from such a critique seem doubtful for the following reasons. The ideas of substance and of cause, taken in an absolute sense, do not constitute *a priori* conditions for the possibility of experience, *such as it is,* in its imperfect and inexact forms. But rather these ideas furnish only the conditions for a *typical experience* that has the value of a strict standard of proof. But that experience is not possible.

The possibility of a logical representation of reality more and more manifested as science grows, does not imply a form of knowledge that is completed in any portion, and so the *a priori knowledge* which Kant desired to conjoin with it is not thus to be demonstrated. We cannot admit the dilemma which Kant sees between the acceptation of knowledge *a priori* and skepticism, precisely because he contrasts an absolutely rational science with absolute ignorance, while the various *degrees* of knowledge can never conform to this rigid distinction. That an experience is possible in general, and in subordination to logical standards, signifies nothing precise, unless we determine what kinds of experience are made possible by the verification of certain conditions or facts.

And this judgment is evidently *a posteriori.* The postulate of

knowledge, understood in its truly positive sense, implies only the *relative* and *not the rigid invariability* of certain objects of experience called real things. The logical representation of reality supposes a theoretically rigid degree of invariability, which in each separate case depends upon the extension of the deductive development. Hence arises an antinomy that is reconciled by limiting the application of logic.

In order to make our recent statements more precise, and to study the question more thoroughly, it will be well to begin with a critique of the ideas of substance and cause.

§ 22. SUBSTANCE: MATTER AND ENERGY.

The idea we form of *substance* or of an invariable substratum of phenomena, expresses our belief that: "In reality certain phenomena coexist in such wise that certain perceptible characters recur again and again in similar groupings which form objects," and hence that "Certain resemblances of characters imply others." This very belief is found to be the basis of the representation of real things by means of concepts, constructed by means of abstraction from classes of elements.

Now the general idea of substance takes on a concrete form through the recognition of certain invariants, which according to the Kantian order of ideas ought to be taken *a priori* in a rigid sense. These invariants are, for modern physics, *matter* and *energy*.

Kant himself gives as an example of an *a priori* synthetic judgment, the principle that: "Throughout all the changes of the corporeal world the quantity of matter remains invariable."

What does this principle signify? If we try to find in it an actual synthetic judgment, we must first of all define quite positively what we mean by the "quantity of matter." And when this has been done, for example upon the basis of "weight," the invariability of matter only expresses the result of Lavoisier's experiments. If on the other hand we adopt the dynamic definition of the quantity of matter (cf. Chap. V), the same statement comes to express a more abstract relation, which results from the comparison of more extended experiments, especially the chemical experiments already mentioned, and Bessel's experiments with the pendulum (the proportionality of the mass to the weight on any portion of the earth's surface).

But all these experiments are merely approximations, and the principle in its exact form adds to them the postulate of a more

exact verification, whenever certain causes of error shall have been eliminated or corrected. What right have we to hold that this postulate *must* necessarily be verified?. Such an assurance is so far from seeming to be universally justified, that certain delicate experiments recently performed by Landolt, and repeated by Heydweiller, tend even to throw doubt upon the invariability of weight in chemical reactions. In a solution of sulphate in water he believed that he observed a perceptible loss, although it was less than 0.000,001.[1]

It is not impossible that a proof of these conclusions may be so brilliantly brought out as to force itself upon us as an ascertained fact. And what should we say then about the invariability of matter?

The principle may be saved, as Lord Raleigh points out, by throwing doubt on Bessel's experiments. This is a plausible hypothesis which however would itself need to be subjected to the control of experiment.

But other expedients are also possible. We can, for example, with Heydweiller, resort to the hypothesis of a varying *radio-activity* of matter. Matter would then no longer be rigidly unvarying in its chemical reactions, unless we add to the result of the accustomed processes of measuring *something else,* which emanates from the bodies taking part in the phenomenon, but which is manifested outside of, and even at a distance from them. But in the first place, this idea changes the meaning of Lavoisier's principle, and in the second place it also shows the impossibility of attaching a very rigid meaning to it, because we cannot limit the radiations of a body to the regions of space that are accessible to experience.

If in any way these latter conclusions are recognized as evident, the *a priori* character of the principle of matter can no longer be defended except by those who regard it simply as a *condition for the definition* of the quantity of matter. In this sense the principle will still retain a synthetic *a priori* value, in case we take as our supposition the existence of an object which strictly fulfils the postulated requisite of invariability.

But just this very transcendental supposition is seen to be without meaning, for reasons already so often repeated. To what

[1] Landoldt's recent communication to the Berlin Academy (1908) states, on the other hand, as the latest conclusion of his researches, that Lavoisier's principle is well verified as within the limits of the inevitable errors of observation. In any case, the discussion aroused by the foregoing researches retains its philosophical interest. (Note added in the 2d edition.)

is a defining condition reduced, when no physically possible object corresponds to it?

From this manner of viewing things there remains however this; that at the very moment when experiment seems to impeach the accuracy of the principle, the partial knowledge already acquired becomes the starting point for new corrective hypotheses, which tend to obtain a more rigorous invariability.

And this attempt is made by seeking to preserve in form the expression of the relation which has been shown to be inaccurate. One thus follows that *principle of formal permanence* in which Peano has seen an *economy* of scientific expressions.

Only from this point of view does H. Poincare seem justified in viewing certain principles as pure *conventions*. Meanwhile for us the truly instructive and heuristic value of the principles themselves lies in the variable content of those hypotheses about reality, which tend to fix the physical significance of the principles, by becoming better determined throughout the progress of scientific research.

And thus the concept of substance illustrates the general tendency to supplement a *slightly variable* relation by assuming some more fixed object as the basis of the variation.

The same conclusion comes out clearly from the examination of the principle of the conservation of energy.

Mayer discovered that heat has a dynamic equivalent, so that he was led to regard as constant, in every *isolated system*, the sum of the kinetic and potential energy in relation to a given quantity of heat.

But the law thus expressed is not verified when phenomena of another nature come into play, as those of electricity, magnetism, light etc. Hence we are led to modify the law, by seeking in these phenomena some element which in the thermic or dynamic transformations shall have a quantitatively invariable representative.

The principle of the theory of energy thus fulfils in a second phase of its development an office camparable to that of a defining condition. But it only gains an actual meaning in a third phase, when we have settled just what is to be understood by electric, magnetic, luminous, chemical and other forms of energy, and when we thus succeed in expressing a more complex and complete physical relation.

We cannot pretend that this relation will ever assume a perfectly fixed value. To note how potential energy (or energy of position) enters into the total energy of a system, is sufficient to show that

no perfectly isolated systems exist. The search for such a system would lead us to consider the whole universe in its infinity. But what sense could there ever be in the affirmation that the energy of the universe is constant, if, as seems highly probable, it is infinite?

To recapitulate, the meaning and value of those principles which are connected with the idea of substance seem to be as follows:

1. The *relative* and *approximate* recognition of a real relation that is *invariable*, or to be more exact, but *slightly variable*;

2. A general confidence that behind the relation just described, as a completion of it, something *more fixed* can be discovered;

3. The concrete assurance that in the various cases this correction may be obtained by taking account of such and such other perceptible data, and may be verified by eliminating by means of averages etc., certain causes of error.

The first statement corresponds to the dogmatic view of acquired science, when we are expounding the results of experiments actually performed. In the second and third statements we find the expression of a will which anticipates the experiment by means of a *postulate* that is always more determinate and significant in its relation to new possible experiments.

§ 23. CAUSE.

All idea of rigid invariability is also banished from the relation of cause when understood in its positive sense. When we say that *cause* is a phenomenon invariably followed by its *effect, certain conditions* are always implied. And if it is convenient to fill out our statement of the cause by taking account of certain *noteworthy* conditions, in which we find the relation of succession, it is clear that we thus make a *choice determined* by the *frequency* or the *interest* of certain conditions in comparison with others. But if we should attempt to carry to its utmost consequences this mention of conditions, the causal relation would lose all significance, because its absolute form would be this: "Every phenomenon is the effect of the preceding state of the universe," from which it would follow that: "To make the exact repetition of the phenomenon possible it would be necessary to reproduce the preceding state!"

It is rather difficult to define the standards of choice which tend to determine the concept of cause for any class of facts. From the abstract logical point of view, we may regard this choice as a free *convention*, (according to an opinion recently maintained in

French philosophy), yet when we consider the more essential aspect of the question, we shall be led to distinguish:

1. Those implicit *assumptions,* by which we assume that the variation of certain elements does not bring about a perceptible variation of the phenomenon under discussion. As for example, when in the study of physical phenomena on the earth, we adopt as permanent conditions the astronomical relations of our globe (although these in certain classes of facts may be contradicted by a comparison of experiences, and in consequence may require correction).

2. The *conditions* by which we arbitrarily narrow down the fact within the limits of certain *interesting* variations, as for example, those which fall within the domain of the will etc.

In physiology, life and the complete state of the organs constitute the aggregate of the conditions we regard as fixed, when we are studying the definite nature of certain functions. For this reason physiological experiment meets with a serious difficulty whenever it modifies these conditions. For precisely this reason the experiments of directly removing the lobes of the brain are difficult to interpret in their relation to localization, because we are obliged to take account of the nervous shock following the operation, and of other functions which follow, to take the place of those lost with the brain-lobes.

In legal science, we tend to limit the notion of cause to that which is connected with human activity, because the law desires to influence that group of activities.

The classic theory of *responsibility* directly appeals to the voluntary nature of acts, whether we are dealing with *fraud* or with *culpable negligence.* At present this view seems to have been outgrown by legal doctrine. When for example there is a responsibility connected with the damage caused by the working of a vast economical activity, we cannot properly speak of *culpable negligence,* except by a legal fiction. Therefore another view seems more adequate which, taking account of the statistical results that are in these cases regarded as a damage not necessarily culpable, treats those results as the direct consequence of the operation of such industrial enterprises. In any case, we must not lose sight of the voluntary factor, whose economic significance the law takes into account.

In other cases, however, we cannot be equally certain of remaining within these limits. And for the rest, as well as the voluntary causes of action, it seems proper to take account of those objective

elements which can act as determining factors (as for example the increase in the probability of injury consequent upon a given action). The problem of responsibility is complex, and the speculations of our own Venezian, and those of the German philosophers Sigwart, Kries, etc. tend to bring it out in various lights. We do not mean to go further with these questions. It is enough to have pointed out the principle that the choice of causes depends upon the *social interests.*

Other observations may be added to these to show how difficult it is to define the concept of cause precisely. Leaving out of account the psychological aspect of the idea of cause, which we shall later find an opportunity to examine, some examples may serve to show us that the causal relation, as commonly understood, implies something more than a mere constant succession. In fact the succession of night and day is not conceived as a causal connection, if for astronomical reasons, we bear in mind the possibility (realized on some other planet) of an eternal day, followed by no night.

The example just given is sometimes interpreted in this way. It is maintained that the causal relation must consist of an invariable and *non-invertible* succession;[1] and that when this requisite is wanting we shall have cases not properly causal. But this very simple criterion seems inadequate for the distinction we have in mind, because for example between the egg and the hen, notwithstanding their reciprocal alternation, we can establish, in both directions, a relation of causal dependence.

It would seem more correct to say that the succession of nights and days though constant on this earth, is not regarded as *invariable,* considering the variety of conditions requisite for such a class of phenomena. In conclusion, the *postulate of causality* may be very nearly determined as follows:

In every class of phenomena, which when suitably limited may be considered as repetitions of the same phenomena, we believe that we can in general recognize sundry groups of reproducible antecedents (causes), from which the given phenomena will *invariably* follow, in case:

1. We observe the repetition of the causes;
2. Or in case we voluntarily secure this repetition.

And the causal postulate will hold true with a degree of *approximation* that depends on the *limits* of the field of observation,

[1] In this way Fiorentino explains the "unconditioned" character which Stuart Mill attributes to the causal antecedent.

or on the degree to which we can voluntarily reproduce the causes. This approximation may become closer and closer in relation to the indicated limits, according as we take account of more and more causes, that is, of any noteworthy antecedents of the given phenomena.

§ 24. RECAPITULATION.

In these recent statements we have made plain the *relative* and *not rigid* meaning of the invariants of reality. The logical representation of reality must then be understood in the same sense. To the approximate physical invariability found in the coexistence and succession of real things, we have the corresponding *rigid* logical invariability of the objects of thought, and of their combination or of their arrangement as elements of concepts.

Thus representation by means of concepts appears, with regard to reality, as an *abstraction* through which a totality or aggregate of objects is *isolated by thought from all complicating conditions,* or factors of variation. And this abstraction implies a voluntary assumption,—the very assumption which we have already seen to be the preliminary to scientific discovery. This discovery is completed by the act of *verification.*

We are free to construct concepts which represent a group of facts, by abstracting from whatever complicating conditions appear to us at first sight to be negligible, and upon the foundation thus obtained, to proceed by deduction. But the relations thus deduced have only a hypothetical reality, for they will prove to be true only in so far as the negligible elements fail to produce any perceptible variation upon them.

The hypothesis that the invariability of the relations of substance and of cause would belong strictly and entirely to the facts conceived of, if they were not isolated from their concomitant conditions,—this hypothesis means only that the errors accompanying all representations by means of concepts were corrected, and the limits of representations are extended so far as concerns the truth of the consequences deduced, in proportion as we take account of more and more of the real elements. The *universality* of the ideas of substance and cause has no meaning apart from this interpretation.

§ 25. THE ACTUAL VALUE OF LOGICAL PRINCIPLES.

The foregoing considerations lead to a discussion of the problem of the real significance of the principles of logic. The question

is an ancient one, and it was already disputed in Greece between the Eleatic school and the Heracleiteans, and in newer forms Hegel and Herbart have argued the same question.

When we consider the principles of identity and of contradiction as referring to physical objects, we are obliged to affirm their objective validity at least from our point of view. To reason about any object implies the hypothesis that it is invariable.

This is the idea of the Eleatic school. This school was naturally led to declare the permanence of the world, in agreement with the permanence of our mental concepts, and both Parmenides and Zeno, by the use of artificial sophisms, deny both change and motion.

But the actual facts were arrayed in opposition to that subtile dialectic in which the finesse of the Greek mind delighted. There was no need of a highly developed experimental science to disprove such conclusions. The school of Heracleitus therefore disputed the immutability of the universe by declaring its perpetual change: *πάντα ῥεῖ.*

Now this seems to be the nucleus of the question. The formal requirements of logical representation express only a psychological fact which cannot *a priori* be applied to the real world of phenomena thus represented. Therefore the principles of logic cannot corroborate any fact that is opposed to change. Nor should they in any sense be understood as the statement of a fact. They express rather the *conditions under which an object or a relation of the world of phenomena may be logically represented as an element of a concept, which the mind has agreed to treat as invariable in forming deductions.*

In reality we find some objects (phenomenal coexistences and successions) which vary rapidly, while others change so slowly that they may be regarded, at least provisionally, as invariants. Now the former cannot be taken as units of logical thought, while the latter, on the contrary, are suitable for such treatment, and the more so, the slower is their rate of change.

"Sir Panera"[1] who had but little skill with his sword, thrust straight at his adversary, who turned aside the blows by a simple parry. "If he doesn't keep still I can't do a thing," cried the poor fellow in his Lombardy dialect.

His regret makes us smile, because we think it is the fencer's duty to choose the brief moment when his adversary is still and may be reached by a rapid thrust.

[1] The allusion is to a Milanese comedy: "Il duello del sor Panera."

Thought is in much the same situation when it tries to discover among the variable elements of reality, an invariable subject for logical reasoning. This logic must discover for itself, unless it would recall by its simplicity the absurdities of Sir Panera.

§ 26. THE VALUE OF AXIOMS: THE OBJECTIVE REALITY OF LOGIC.

Let us suppose that in a given group of facts we have found certain quasi invariable elements, so that the conditions for representation as expressed by the principles of logic, are found to be relatively fulfilled. In order that the representation may be adequate, it is necessary that the axioms which express the laws of logical associations, shall find their equivalent in reality.

But this actually happens, at least within certain limits which depend upon the manner in which the premises are realized. In fact the axioms find their equivalent in the actual properties of the aggregates of objects. For example, the concrete previsions correspond, in this field, to the formal properties of arithmetical operations.

We are thus led to perceive that the application of logic supposes the realization of a general fact:

Under the condition of invariability expressed by the principles of logic, aggregates of objects satisfy the requirements of the axioms. In this sense we may speak of the *objective reality of logic,* which implies an invariable and *conditioned* relation between the laws of thought and the world of phenomena. And this relation is truly significant. If we may imagine a thinking being in whom the associations of objects perceived by the senses as fixed take place according to different laws from those expressed in our axioms, we must admit that such a being could find no correspondence between his logical process and outer reality.

Illogical men resemble such a type in this respect, that in associating the images of the objects of sense-perception, they do not succeed in inhibiting the modifying effect of the ideas or sentiments called up by the representation of the composite object.

But up to what point and in what way may the fact affirmed by the objective reality of logic be regarded as verified? The most direct and familiar verification of the properties of aggregates is controlled by this supposition which science adopts, and indirectly confirms during its progress:

If in a class of objects, apparently fixed, the properties expressed

by the axioms are not verified, more perfected means of observation will inform us of the variability of these objects.

This vague formula acquires, in separate cases, a determinate meaning, when we succeed in fixing quantitatively a lower limit to the supposed variation. And in this sense the principle comes under the control of experience, which has never failed to confirm it, as far as possible.

But in relation to scientific construction, in which experience is logically arranged, our unlimited confidence in this postulate constitutes a condition of progress. This progress thus requires a postulate, which shall complete and define that postulate of knowledge, which we have the right to maintain until we perceive some necessary halting place of progress. Such a halting place is not in any way forced upon us, and we cannot even imagine it to be possible.

§ 27. THE LIMITS OF THE APPLICATION OF LOGIC.

We must further insist upon this point, that the properties of assemblages of objects require a certain invariability in the latter, since this observation marks the limit of the application of logic.

Let us once more take up the example of concrete arithmetic, regarded as a branch of applied logic. We have regarded this as a matter of thought, but cashiers think differently. They count their money by dividing it into piles, add up the amounts and check their calculations in various ways. The same predicted sensations are always produced under these voluntarily arranged conditions, so that there seem to be arithmetical facts which the cashiers find to be constant. If we take twelve coins from a pile, they may with equal ease be placed in two groups of 5 and 7, or of 6 and 6. This is the objective sense of the formula $7+5=6+6$.

Meanwhile these arithmetical facts, subordinated to the general properties of assemblages, suppose conditions that are usually implicitly taken for granted, but which are none the less necessary.

If some good cashier who was fond of hygiene and cleanliness, were in the habit of putting the coins in water while he was figuring, experience would still confirm his arithmetical previsions. But woe be unto him if a fellow clerk should take a fancy to play him a trick by substituting for the water some acid which attacks metal. In this case his calculations might "come out wrong" because a coin might disappear in solution while the poor man was busy with his logical deductions.

But there are more important reflections suggested by the prin-

ciple that "the general properties of assemblages suppose a certain invariability in the objects associated" when we see that this principle sets limits to the application of the logical process to the knowledge of reality.

We are thus led to perceive the difficulties that stand in the way of the representation of different classes of facts by means of deductive theories; or in other words we recognize up to what point the logical process of deduction may be applicable in the various sciences.

By seeking, in the midst of changing phenomena, the invariable groupings of these phenomena, our research aims to determine what real things may be assumed as the objects of thought, or may be represented by means of their relations in a system of concepts. But the invariability of real objects being only relative, the logical prevision of the properties belonging to their assemblages is confirmed *only up to a certain point,* that is in case of the first reasoning processes, for when the deductive process is carried further, the variations accumulate and hence the prevision is falsified.

The slight variation of length which the pendulum of a clock undergoes by the influence of temperature is indeed negligible, so long as we are only trying to count a certain number of seconds, but in a greater length of time a difference will thus be produced between one clock and another. So that finally we have the sums of an equal number of equal terms, which nevertheless differ from one another!

Every science which verifies its deductions, can fix *a posteriori* the limits within which its objects can be logically represented. This we shall see in dealing with the testing of implicit hypotheses. These limits may be wider or narrower according to the relative fixity of the phenomenal relations logically represented.

Upon what does a fuller use of the deductive method depend? Upon the fact that the phenomena used as data show very different degrees of variation, so that by means of abstraction we may reach more fixed relations which are relatively stable. Again the employment of deduction depends upon the degree of invariability in the data assumed as fixed. This degree varies with the previsions that we want to make.

These conditions are best realized by physical phenomena, and especially by those which are included under the most advanced theories. In this field the process of the derivation of functions has precisely fulfilled the requirement already referred to, of iso-

lating something comparatively fixed from more variable elements, as for example in the passage from Kepler's theory to Newton's (Chap. II). For this reason the theories of physics tend to express the most constant portion of the quantitative relation of phenomena by means of differential equations.

In chemistry the search for invariants to use as the objects of logical treatment is seen in the repeated efforts to isolate undecomposed elements from the other phenomena, and then in the separation of certain compounds or fixed nuclei in a certain order of reactions. The atomic symbolism, with its structural formulae, precisely corresponds to the hierarchy of the invariants.

On the contrary in physiology the more rapid mutability of data, or if you will, the greater complication of causes, renders the office of the deductive method more limited. While astronomy can isolate the planetary system from the rest of the universe, during the brief period with which human history deals, physiology cannot disregard the general influence of the whole organism upon each organ, if it attempts any previsions which shall be valid for more than a few moments. Minutes in animal life count as millions of years in the life of worlds!

We should add that the greater or lesser variability of the data that form the object of a science, ought to be understood not only with regard to time, but also with respect to those associations which are taken to be simultaneous. In physics many characters, such as mass for instance, are combined quite unaltered when different bodies are united. But in physiology, and in the social sciences, such a union always makes more alteration in those component parts which fall under our observation.

As a matter of course all these statements are to be taken in a relative sense. The possibility of extending the realm of deduction exists in every science, but it must be controlled by the choice of the more invariable data, that is, by the verification of the implicit hypotheses which affirm this invariability.

§ 28. THE PROBLEM OF VERIFICATION.

We now propose to discuss the second fundamental problem of applied logic, which deals with the verification of theories. And we must especially bear in mind, from what we have already said, that the representation of reality by means of a system of concepts may be expressed in two classes of hypotheses:

1. The *implicit hypotheses* by which we choose certain group-

ings of phenomena (the invariants), treat them abstractly as elements of concepts, and then regard them as equal to one another.

2. The *explicit hypotheses* (*the postulates of a theory*) which, by means of associative relations defining the concepts themselves, express the objective relations between the invariants designated.

It is plain that the formulation of a theory brings to light only hypotheses of the second species, while those of the first class are held to be previously accepted as something already acquired.

Thus, for example, we see in physical theories, certain quantities, as temperature, pressure etc., expressed by certain equations. These equations express explicit hypotheses, but the supposition that the quantities mentioned are defined in relation to a certain corporeal state etc., represents the sum total of various implicit hypotheses. We can realize the degree of extension of which the latter are capable, if we consider the variety of experimental processes which may be needed to determine them.

§ 29. THE VERIFICATION OF EXPLICIT HYPOTHESES.

Now the problem of verification leads us to deal differently with the criteria that apply to the explicit and to the implicit hypotheses.

While we are verifying the former, the latter give us some knowledge beforehand which limits the field of *experience* and enables us to interpret it. Thus we find ourselves face to face with a world of phenomena, whose objects we suppose to be already associated in certain classes, series, etc., which are determined by certain representative concepts.

The hypotheses to be verified having been already transformed by means of deductive reasoning, we must submit to experimental control certain general propositions or theorems of the theory, which may refer to:

1. The characteristics of classes; that is, characteristics which are regarded as present equally in various objects of experience.

2. Or characteristics susceptible to change as *functions* of the object, within the class itself.

1. It is plain in what way the particular experiment instructs us in the former case, in which the implicit hypotheses contain the possibility of making generalizations. This is just what happens, for example, when we are trying to determine the anatomical features of a species of animals, the different representatives of which we regard as alike.

Meanwhile the value of the corroborative experiments varies,

according to whether we are dealing with (quantitative) characteristics that are liable to change by insensible degrees, *continuously*, or with (qualitative) characteristics which we conceive only as elements in a *separate* class of possible objects, since the former characteristics can be determined only *approximately*, and the latter *exactly*.

In the example just mentioned—the anatomical characters of species—we are dealing with qualitative characters, and the answer given by experience is exact. We find another analogous example in the determination of the type to which the crystals of a certain mineral belong, and the like.

On the contrary the determination of the melting-point of iron, or copper etc., gives us an example of a quantitative character of classes, which experiment can furnish only approximately. Approximate knowledge can be changed into exact knowledge if we are speaking of a limited interval within which is comprised the value of the quantity experimentally defined.

2. When we wish to find out how a certain character is distributed amongst the members of a class, we usually come to the more special inquiry of the following form, namely, "To find out whether a certain element is constant for all the objects of a class; That is to say, whether it may be considered as a characteristic of the class."

We will explain this reduction by an example. If a force f acting on a moving point in its different successive positions, is inversely proportional to the square of its distance r from a fixed center, the product fr^2 has a constant value for the successive positions in question, and to verify this constant ($fr^2 = \mathrm{K}$) is equivalent to verifying the dependence

$$f = \mathrm{K}/r^2.$$

Now the question of ascertaining by experience whether a certain character is constant for all the objects of a class leads to considerations that differ in part, according to whether we are dealing with discrete or with continuous classes. This character could itself be regarded as variable by separate degrees, or continuously (the qualitative or quantitative characters), and in this connection we had best repeat our former reflections as to the exactitude or the mere approximation of particular experiments. But the *generalization of experience* forms the chief difficulty to be examined, and this difficulty is connected with our former distinction between experiences that are quite separate and those that are continuous.

§ 30. THE EXPERIENCE OF A FINITE NUMBER OF OBJECTS.

Among the separate classes we find in the first place those composed of a finite number of objects. In case their number is not too large, and if the objects are accessible, we can obtain a *complete verification* of our hypotheses, by trying as many experiments as there are objects. This is the most obvious case, though it is of little practical interest.

Let us on the contrary suppose a discrete class composed of a very large, or even of an infinite number of objects. Only a certain number of experiments are actually practicable. These experiments may lead to either one of two results:

1. That a certain property which we are investigating to see whether it is a character of a class, does not belong equally to all the objects observed. In this case the answer of *experience* is *certain* and *negative*.

2. Or that the property observed does belong equally to all the objects observed. What justification then has the argument that it must belong to *all* the objects in the class? That is, in how far is it proper to interpret experience in the sense of a *positive* verification of our hypothesis?

Let us examine this fundamental problem. Let us imagine that we are at a lottery where we draw folded slips of paper out of an urn. Those marked with a number correspond to the prizes. The possibility is not excluded that all are certain to draw a prize.

We draw twenty or thirty slips from the urn, and they all have numbers. We then conclude that *probably* we are sure of a prize. If on the contrary we had drawn an equal number of blanks, we should merely think that there were only a few prizes. How do we come by this difference of interpretation?

Because we had reason to suppose (from having observed other similar lotteries) that the numbered and the blank slips would be mixed, and that the blanks might be very numerous in proportion to the numbered slips, but not *vice versa*. And the case of a fraud, with no prizes at all, must appear extremely improbable in comparison with the case in which the prizes are certain.

We see then that the interpretation and generalization of experience seem to be subordinated to certain assumptions regarded as true. And it is only when these assumptions have been made definite in the form of a coefficient, that it becomes possible to *measure the probability* of the result, that is, to determine a number

which shall express in how many out of a large number of cases the prevision obtained by generalization will be correct, and in how many it will be erroneous.

If there is no presumption, the different cases may be regarded as *equally probable.* This hypothesis would then furnish at once a roughly approximate first criterion for the interpretation of experience,—a criterion to be corrected with the progress of knowledge. And it is well to notice that when we are obliged to adopt this criterion, the value of the resulting generalization will be but small, as soon as we are dealing with classes of objects that are very numerous in comparison with the actual number of our experiments. What then shall we say in case of classes whose objects are infinite in number?

§ 31. EXPERIENCE OF THE CONTINUOUS.

The preceding observations will show us that when we are dealing with a continuous object our verifications will be more and more necessarily subject to the assumptions used.

For the rest, we can here recognize a general criterion of interpretation, in accordance with ideas already pointed out. This is that assumption of *continuity* which lies at the basis of the *experimental method.*

When we are engaged in qualitative experiments, since the characteristic feature of a class cannot vary continuously, it is *a priori* regarded as constant, at least for a *part* of the class contained within certain *limits* which separate it from an *adjoining class* (as for example for a liquid between the temperatures of boiling and freezing).

But let us turn to the case of quantitative experiments. Suppose for example that we are trying to verify an equation of the type $a = b$ for the objects in a continuous class, that is (the approximate character of such an experiment being granted), we are trying to ascertain whether there is an inequality between these objects

$$a - b < \epsilon,$$

in which expression ϵ represents the *error of observation.*

The verification is accompanied by *interpolation.* That is—dealing with a class of *one dimension*—we experiment upon some objects, and extend the result to other objects comprised between these. On the basis of the presumption that the relation between a and b depends *continuously* upon the elements of the class, we con-

clude that this relation must be *constantly* $a=b$, if this result corresponds with the objects experimented upon.

The principle that serves to give us a measure of the criterion of interpretation—that is, to show in how many cases the generalization leads to correct previsions— is still the principle of *probability,* according to which we accept what is called the *law of large numbers.* But the *mathematical probability,* to which (according to this law) the number of cases for or against a given hypothesis is proportional, is not defined unless we have first classified the variety of possible cases, in definite ways.

Let us explain how the criterion of probability aids us to estimate the results of experience. We will make use of a simple example.

Let us propose to measure the amount of heat that can be developed by a certain mechanical work of a kilogrammeters. *Let us suppose* that the quantity mentioned depends only upon the variable a and is a continuous function $f(a)$ of this variable.

If a first experiment gives

$$f(a)=a:427$$

an experimentor who was not prepossessed with any notion on the subject could infer nothing from this result.

But let us suppose that a second trial experiment gives again the same number, or else that the result of the first experiment had been previously stated.

In such a case, every one will admit that the experiment has this value, that at least within certain limits, the ratio $a:f(a)$ *has the constant value* 427. Upon what is this confidence founded? In case we should deny the thesis, the function $f(a)$ could be represented by the following diagram.

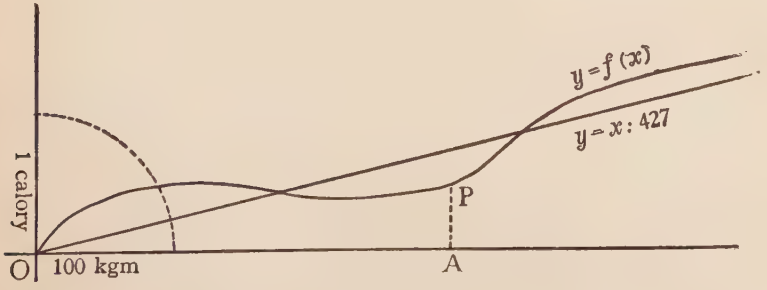

Fig. 3.

Now if we take at random upon the axis x a point $A(OA=a)$ what probability is there that the ordinate $AP=f(a)$ will have the assigned value $a:427$?

Plainly this probability is *infinitesimal,* or practically nothing. But if we regard the experiment as exact, there is an *infinite probability,* or a *practical certainty* that the continuous curve $y=f(x)$ coincides with the straight line whose angular coefficient is $1:427$ or that for a finite distance it coincides with this line.

But let us bear in mind that the value 427, obtained by experiment, is only approximate, and therefore let us regard the experiment as subject to a certain error $<\epsilon$. Then the foregoing calculation must be modified in this sense: What probability is there that a point taken at random upon the curve $y=f(x)$ will diverge from the straight line $y=x:427<\epsilon$?

It would seem at first sight that we should thus be merely drawing a conclusion similar to the former one, that is, that by taking as a proof a probability no longer infinite, but very great, we may regard as proven by our experiment the approximate coincidence of the curve $y=f(x)$, or of a part of it, with the straight line $y=x:427$.

A mathematician versed in the study of functions with many singularities would be of the opposite opinion. He would not find much difficulty in constructing a continuous curve $y=f(x)$, which at short intervals should approach and diverge from the designated straight line as much as he chose.

That only proves that the probability cannot be mathematically estimated so long as we allow the continuous function f to be entirely arbitrary. We should rather make our problem determinate by limiting this arbitrary character on the basis of an assumption regarding the phenomenon.

In the case we have chosen for an example, we must admit that the function $f(x)$ is an *increasing* function, a fact which at once excludes certain oscillations of the curve.

We will not here carry out our analysis to the concrete determination of this probability. It will suffice to point out how the criterion referred to can permit us, at least in the simplest cases, to *estimate quantitatively the probable value of experimental verifications in relation to some sufficiently determined assumption as to the character of the phenomena.*

Naturally as we increase the number of verifications that relate to closely neighboring facts, the value of the proof by *interpolation*

increases rapidly, while *extrapolation,* which grows more distant from the realm of experience, loses more and more of the value of proof, its office being gradually reduced to the simple suggestion of new hypotheses.

§ 32. CONCLUSIONS AS TO THE INTERPRETATION OF EXPERIENCE.

Let us sum up the results of our foregoing critique in the following conclusions:

1. The value of empirical verification, or the interpretation of experience, depends upon the concepts used, and in particular upon the implicit hypotheses contained in them.

2. If we are considering properties accepted in advance as belonging to classes, the experience has a *value which is certain,* and which is *exact or approximate* according to whether we are considering *qualitative* or *quantitative* facts of experience.

3. Qualitative experience of continuous objects always deals (within certain limits) with the properties of classes, if we accept the general assumption of *continuity.*

4. When we deal with discrete objects or with quantitative facts belonging to a continuum, and have to discover "whether a property is common to the objects of a class," then experience gives us *certain* results if the results are negative. *Positive* results have only a *probable value,* which can be measured if we accept the law of large numbers, and assume a type of conception fitted to the various fields of experience (implicit hypotheses).

And so the criteria for the verification of explicit hypotheses are based upon implicit hypotheses and upon two general principles: the law of large numbers (probability) and the *postulate of continuity.*

If we are asked to justify principles, we can only refer to the whole mass of experience collected.

We must then admit the general assumption that experience gives a probable warrant for generalizations. And therefore we must regard the standards for measuring this probability, and the postulate of continuity with which they are connected, as an enormously probable result, by reason of the vast number of verifications, direct and indirect. The progress of science which is furthered by these very principles, serves every day more and more to confirm them and to render them *determinate.*

§ 33. THE POSTULATE OF CONTINUITY AND THE PSYCHOLOGICAL REPRESENTATION OF CAUSE: HOW AND WHY.

It is now important to notice how the principle of continuity corresponds with the psychological aspect of our ideas of substance and cause.

We shall especially refer to the latter, and so take up again the connection between the antecedent and the consequent regarded as cause and effect, considering them from the psychological point of view.

These questions, so much discussed by metaphysicians seem on the other hand to be rather neglected by the positivists, partly because of the transcendental sense which has been attributed to them, and partly because of the metaphysical search for a so-called first cause, or an absolute, unconditioned or efficient cause.

The significance and interest of this question clearly appear in scientific progress, in which we observe that:

1. In science all causal explanations cannot be regarded as of equal value, for in some we find only an answer to the question "how can this phenomenon be produced?" while in others we can see—relatively understood—the "why" of the phenomenon itself.

2. The different value of these two kinds of causal explanations can be estimated in a positive sense as to the extent of knowledge which they make possible, in view of the fact that the knowledge of the "why" makes possible a wider range of prevision, especially in relation to the possible variations of those elements which enter into the constitution of the cause.

Let us look at a simple example: We strike a plate of copper with a hammer. The copper grows hot. This statement tells *how* copper can be heated. But *why* does it grow hot? We must ask this question of a physicist who understands the mechanical theory of heat. He answers: Heat is a state of vibration of the particles of a body, and the motion of the hammer which is suddenly stopped by the copper, is transmitted by the blow to the particles of copper, which thus begin to vibrate.

Of course such an explanation does not pretend to point out an ultimate cause of things. It reduces the question to still another, which remains unanswered: "Why is the motion transmitted by the blow?" But precisely the reduction thus obtained gives us an intellectual satisfaction of its own, and in this connection we find

a fuller knowledge of the ways in which bodies may be heated by mechanical means.

Science never requires an absolutely final answer to its questions, but is provisionally satisfied with similar answers to its "why," being able to follow the series without necessary limit, and to gain at every step a similar advantage.

As a further example, let us cite the explanations of the different chemical processes by which zinc sulphate and copper sulphate are formed,—an explanation which brings us back in the last analysis to the elementary electrical attraction between neighboring particles of matter, an attraction of which we cannot pretend to give any further explanation.

Now if we wish to examine more closely the difference between various kinds of causal explanations, we may make the following general observations:

When we point out *how* any thing happens, we are stating a simple relation of succession between an antecedent and its consequence, taken by themselves. When we state *why* anything happens, we associate with this statement the conception of a *continuous series* of intermediate phenomena, either real or fictitious.

This latter explanation then replaces the given causal relation by a *series of immediate* or *elementary causes, subordinated one to another,* a series in which we conceive of the elementary cause as an antecedent very near to its effect.

Hence it follows that the *search for the why implies an idea of causation, and this implies the continuity of causal connection.*

This postulate of continuity is what remains of the elementary causes when we have dropped out the fictitious intermediaries between the actual antecedents and their consequences.

We have already told how this postulate may be justified upon the basis of a wide experience, and how in its own turn it furnishes the most important standards for the interpretation of the process of verification.

We have thus shown the office of the tendency of the mind to look for elementary causes,—a tendency which appears to be in some measure a condition of scientific progress, and which attains more and more success as such progress occurs.

But how far can we succeed in justifying this tendency in its relation to reality?

Upon our earth phenomena of light are produced in connection with light emanating from distant stars. In this case cause and

effect are phenomena separated by millions of millions of kilometers and by hundreds of years.

At lesser, though still enormous distances, the attraction of the heavenly bodies is felt within the planetary system in a manner which we have no positive reason to regard as not instantaneous.

But ever renewed efforts at hypothetical construction appear, whereby we attempt to unite these phenomena by means of fictitious intermediaries.

Light, since its propagation takes time, is conceived as supported by the ether, which is regarded as a medium through which the phenomenon is continuously transmitted. We seek for something analogous as a medium for transmitting gravitation, and hence are sure that a more thorough research ought to reveal to us that the phenomenon of gravitation is not transmitted instantaneously.

We might easily feel tempted to smile at such an argument. What a lack of respect for experience, to which we daily pay homage. Yet the justification of this tendency is to be found in a larger comparison of other instances where experience has progressively confirmed the corollaries of analogous assumptions. And another justification is found in the fact that *to conceive of elementary causes extends the field of possible experience, and gives to this experience a value which would less plainly appear were it not for the hypotheses contained in such an extension.*

§ 34. THE CONFIRMATION AND VERIFICATION OF THE IMPLICIT HYPOTHESES.

The difficulties that belong to the verification of the explicit hypotheses in the various theories, have been partly reduced to the difficulty of accepting the implicit hypotheses. Thus we see the need of discussing the standards whereby the latter may be confirmed, verified or corrected.

Let us first speak of their *confirmation by anticipation.*

In constructing a theory it is possible to assume as implicit hypotheses, the conclusions of other theories that have been more exactly verified. But if even these have not been entirely confirmed by experiment, it often happens that they have been *partially* confirmed and that these partial conclusions, when compared with a larger group of observations, acquire a highly probable value, that is sufficiently exact to serve as the foundation for a new and more solid construction.

In any case the implicit hypotheses thus founded upon *deduction,* can often be completed by means of new combinations of empirical data, that is to say, by *induction.* In the earlier phases of scientific construction this properly inductive process plays a more important part.

In connection with the standards to be used in such inductions as we have pointed out, it is well to have in mind the *canons* of Stuart Mill, founded on the principles of substance and cause. (The English philosopher lays more stress upon cause.) But after the remarks in §§ 22 and 23 it is needless to add that these canons have only a schematic significance and but a limited value.

Thus for example, when we speak of *the inductive method of agreement* which "takes for the cause *the only* circumstance common to the various occurrences of a phenomenon," we always understand that there is an implied *abstraction* from those endless common circumstances that are regarded as indifferent. And the example furnished by the assertion that "the physical state of bodies is due to heat," shows us precisely the limited value of the method, since among the elements here neglected we find "pressure," which must be reckoned with in dealing with the liquefaction of the so-called permanent gases etc.

It follows from the foregoing observation that, however the implicit hypotheses used in forming a theory may have been previously confirmed by an accurate standard, and especially in the early stages of a scientific construction, there is always a doubt as to the value of these hypotheses and as to the limits of approximation within which they may be considered true. Hence arises the necessity of testing *a posteriori* whether and to what degree they are valid, and of rectifying them wherever this is possible.

The verification of the implicit hypotheses can also be accomplished by experience, but rather in a *negative* than in a *positive* sense. If a phenomenal relation that is taken implicitly as an invariant cannot be considered as such beyond a certain range, the fact that this relation is not invariable is shown by a contradiction with experience.

Those logical principles which express, as we have said, the conditions under which the deductive method is applicable, thus give us *a posteriori* the means of recognizing the *error* of the implicit hypotheses, *in relation to a certain system of deductions.* We should hold, on the other hand, that the *agreement with experience,* which

verifies the theorems that logically depend upon a theory, gives a growing confirmation of the implicit hypotheses.

As to the *correction* of the implicit hypotheses that are found to be erroneous, we can point out no other way than to resort once more to the criteria of inductive proof, while taking account of the partial and probable data, which, in spite of the errors referred to, will be given us by the development of the theory. The only general rule we can give is to increase as much as possible our combinations and comparisons. The estimation and comparison of hypotheses as related to that totality of science which we provisionally regard as actually acquired, constitute the only way of judging of any principle that is to be found at a given time in use as the basis of a scientific structure.

We may add that the simple multiplication of "controls" is not the only means by which scientific progress can succeed in accomplishing more accurately the verification of the implicit hypotheses of a theory. While *deduction* increases the number of verifiable theorems, *definition* frames new concepts which represent new possible relations of real objects, relations which are to be taken as invariants. Hereupon a slight variation from fact in the relations originally taken as the objects of thought, becomes an appreciable variation in the relations that have been defined as a consequence of the investigation. And this variation thus becomes subject to the control of a more exact verification, whether direct or indirect.

Thus definition appears as a process that multiplies the errors of observation, and presents the theorems belonging to a theory in a suitable form for the verification of the premises; while the contradictions arising from the implicit hypotheses can be the more plainly seen.

§ 35. EXAMPLES.

We will give a few examples to illustrate what has been said.

A contradiction with experience is the test that often tells the chemist that a body which he has implicitly regarded as one known simple substance, contains, on the contrary, some other element. In fact it is in this way that some of the new simple substances or elements have been discovered.

In mechanics, the postulate of the invariability of mass in relation to motion is an implicit hypothesis that has been shaken by no appreciable contradiction with experience. But if there is to-day some discussion about this invariability, it has arisen because side

by side with the actual motions there have been defined certain possible motions of an enormous velocity, and these motions have been compared with other orders of phenomena (Chap. VI).

The extension of certain representative concepts by means of definition, offers us a final standard for the correction of the implicit hypotheses.

§ 36. THE PRESENT CRISIS IN POLITICAL ECONOMY.

It is interesting to bring into connection with the foregoing thoughts, the present crisis in political economy, in which the different interpretation of the implicit hypotheses adopted by the classic school appears to cause dispute as to the methods of this science.

It is well known how in connection with this school Adam Smith created the type of the *homo œconomicus,* a type of man whose motives considered in relation to the exchange of capital and labor, were defined as invariable.

This type, although it is considered apart from those various ethical motives that modify and complicate human activities, still corresponds well enough to the representation of the latter, when we have in mind those economic relations, to which the ethical complications are somewhat foreign, at least *in the average* of cases.

In order to define the concept of the *homo œconomicus,* the classic economy has assumed certain simple and almost self-evident explicit hypotheses, from which it has deduced concrete and general previsions belonging to different classes of facts.

We meet with these hypotheses in Ricardo's theory of differential rent. Referring for example to the grain market for which Ricardo's law was originally established, let us suppose that the cost of production in a group of fields can be estimated by reckoning the labor and capital required for cultivation, and the cost of transportation to a given market. Let α, β, γ.... represent the average cost of wheat on the lands in question.

If we suppose that all the grain is sold at the same time in the same market, and if we let *a, b, c*.... represent the average price of the respective sales, we shall get these results:

1. No purchaser is disposed to pay more for his grain than what is asked by the cheapest vendor.

2. No vendor is disposed to part with his grain for less than what is offered by his best purchaser.

In consequence all sales tend to reach a level, and the hypothesis

$$a = b = c = \ldots .$$

may be accepted as very near the truth.

3. On the other hand, no one wants to cultivate land at a loss, so that we must believe that if any one of the differences $a - \alpha$, $b - \beta$, $c - \gamma \ldots .$ were negative for a length of time the raising of grain on that land would cease.

If we suppose the invariability of the prices $a, b, c \ldots, \alpha, \beta, \gamma \ldots,$ or in other words if we refer to a supposed *economical* equilibrium, we shall then have

$$a - \alpha \gtreqless 0, \; b - \beta \gtreqless 0, \; c - \gamma \gtreqless 0 \ldots .$$

Therefore we deduce the proposition that every producer profits by a return equal to or more than the difference between the cost of producing the grain on his own land and the cost of production on the least remunerative land.

In fact, given

$$\alpha > \beta > \gamma > \delta \ldots ,$$

we obtain

$$b - \beta \gtreqless a - \beta, \; c - \gamma \gtreqless a - \gamma \ldots .$$

This deduction concerning differential rent—in the field of agriculture and more generally in any sort of industry—is completed by means of a further deduction founded upon analogous hypotheses.

Let us suppose that:

4. There are certain tracts of land as yet untilled, such that the cost of producing grain upon them increases almost continuously from the minimum α, which belongs to the least remunerative of the cultivated fields.

Then it is presumable that if the price of grain rises above α, that is if we have $a - \alpha > 0$ (and not $a - \alpha = 0$), some one will be found who wishes to grow grain on some of these lands.

Thus on one side we shall see an increase in the cost of the production of grain on the less profitable lands (since people do cultivate lands that are less and less remunerative), and on the other hand we shall see the price of grain diminishing when a greater quantity is put on the market. Therefore we come to the conclusion that "in a state of economic equilibrium, the price of grain tends to become equal to the cost of production on the least profitable land."

This statement expresses the law of differential rent in its most exact form completed by the law which makes the value of the goods commensurate with the cost of production of the product

which stands last in the series. And evidently this statement may also refer to other goods produced by other industries under various conditions.

Now the law thus established has been, and still is, verified with sufficient approximation in many cases, if we bear in mind that the supposed economic equilibrium is a limiting state never fully reached by society.

But on the contrary other observations are found to be in direct contradiction with these verifications, and we may add that the cases in which the law is not confirmed are constantly increasing at the present time.

Thus it is seen that the invariability attributed to the *homo œconomicus* is not so very rigid with regard to certain deductions and the previsions that follow from them.

The variability of this type especially appears in the striking phenomenon of the aggregations of capital and labor. The union of many men, instead of acting as a simple aggregate, varies from our prevision by developing an activity which discounts and prevents the effects of free competition. The recent international trusts are a striking case in point.

Let us see how such associations succeed up to a certain point in reversing the motives that act on the producers. While the single producer cannot increase his profit except by increasing the quantity of his product, the society which has brought up a part of the means of production can better attain its object by so diminishing the product as to diminish the output of goods on the market. For this society only desires to operate in the least expensive ways so long as it is compensated by increasing the profit of the differential rent which it thus obtains. Furthermore such a society can control the market by hindering the production of goods by outsiders, through the threat of offering its own goods at a lower price, so as to cause the ruin of its adversaries, even if this involves some temporary sacrifice.

Especially in the latter psychological phenomenon we find a new element which distinguishes a trade combination from a "group of men." Thus the phenomena just spoken of do not seem explicable if we maintain the individualistic concept of the *homo œconomicus,* even if we modify some of the explicit hypotheses stated above.

We see in the preceding example the two criteria for the verification of implicit hypotheses at work. The contradiction between the various observations shows on the one hand that the invariable

quality of the *homo œconomicus* is not rigid, in the sense required by the previsions. And on the other hand, this non-variability is a direct result of the economical associations, which should be theoretically defined as a simple union or combination of men.

We cannot now draw from these preceding considerations any conclusion which shall pretend to reconcile the strife of the schools of economy. We must be satisfied if we have shown how this conflict (if we leave out the non-scientific elements) is inherent in the process of estimating implicit hypotheses in science.

From this point of view the school which, accepting the historical method, refuses to employ the deductive method, and the mathematical school, which on the contrary develops and perfects that method and passes over from a *static economy* to a *dynamic economy*, may be equally regarded by the philosopher as collaborating in the creation of a new phase of the science.

Perhaps this phase is already outlined in the recent developments of the mathematical school of Lausanne, and especially in the recent works of M. W. Pareto (*Manuale di Economia politica*).[1]

The principal conclusion which seems to result from these studies may be formulated as follows:

We can construct several *economic theories* which differ from each other, by taking as a basis the notion of men's "tastes" and of the different sorts of "obstacles" that stand in the way of their satisfaction. We thus obtain possible representations of those economic phenomena to which the classic theory expresses a first approximation.

Now an economic theory could be so framed as to connect the facts by strict laws once we should have admitted certain fundamental relations (the postulates of the theory). These relations will vary according to time and place, as the historical and psychological conditions of different environments vary.

Thus it is that the deductive method, which has been brought to greater accuracy by the application of mathematics, vindicates all its rights in these closed realms. Meanwhile we recognize history and psychology as the source of those inductions by means of which we attempt to explain the variations in the social environment which lead from the realm of a certain theory to that of another. At least

[1] Milano, Societa editrice libraria, 1906.—The remarks contained in the Italian text have been modified as a consequence of the publication of this work. (Author's note.)

this is the interpretation which we see in the recent developments of economic science.

§ 37. CONCLUSIONS: THE VICIOUS CIRCLE IN SCIENCE.

Let us examine in their totality the criteria whereby the explicit or implicit hypotheses of the theories are to be verified. We shall soon reach a paradoxical conclusion, which may be called the *vicious circle of science.*

The verification of the explicit hypotheses requires an interpretation of experience subject to the implicit hypotheses. In their turn the latter are *in great part* verified by means of other theories and explicit hypotheses, and are corrected when needful by a wider comparison with the knowledge already acquired. Thus the progress of science is dependent upon science itself, and is an *extension* and not a *creation.*

Kant's desire to avoid this vicious circle led him to his doctrine of the *a priori.* And for the same reason this doctrine is born again in a new form in the French contemporary philosophy, which regards the principles, the canons whereby experience is interpreted, as *pure conventions.*

Thus men of learning would be acting like legislators who assign to gold the rôle of legal tender and hence make it a standard of value. Or rather, to carry out the comparison, science would be a state where pluri-metallism is in vogue, since we are not concerned with one single principle.

But the choice of gold for money is based upon the fact that it already has an exchange value commensurate with that of other merchandise. And as to bimetallism, the legal ratio has the effect of substituting the worse for the better money.

Just so in dealing with scientfic principles we are constantly obliged to consider them as the results of experience, and to judge of them by constantly reexamining them in their relation to new experience. This point did not escape the attention of Poincaré. But if we wish to regard things rather from the actual than from the formal point of view, it seems wise, as we have pointed out, to class these principles as *assumptions gradually becoming determinate* rather than as *conventions.*

In any case the whole difficulty remains for any one who does not recognize any *a priori* principles whatever. We cannot escape from the conclusion that experimental reasoning extends science but cannot create it. We must accept this conclusion as a positive

fact, and we will show that this fact logically interpreted does not involve that vicious circle which we have chosen to present as a paradox.

In fact, throughout the process of scientific construction, we always presuppose a certain aggregate of known results, more or less probable and precise, which pass for established. In the same way in every experimental research, and whenever we are forming new concepts, we gain a certain support from concepts already formed.

Thus the approximate invariability of bodies is a fact that is known in advance of the researches of the physicists concerning the constancy of mass. So also certain concepts or intuitions, for example those of geometry, mechanics, physics, are found to be already contained, at least as germs, in our minds, at the very moment of beginning a scientific research, and so these concepts form as it were an acquisition due to the habitual experience of the past.

The vicious circle of science thus turns into a corroboration of the fact that scientific development has no beginning, but extends indefinitely into the past as into the future, and that the acquiring of knowledge does not belong solely to the realm of clear consciousness and of the will, but that it is also extended by the unconscious and instinctive associations.

Now the belief that the latter are in some way adapted to life suffices to give a certain probable and approximate value to the earliest knowledge that we regard as acquired.

The progress of science is a process of *successive approximations,* in which new and *more precise, more probable* and *more extended inductions* result from partially verified *deductions,* and from those contradictions that correct the implicit hypotheses.

In this process certain primary and general concepts, such as those of geometry and mechanics, give us some guiding principles that are but slightly variable if not *absolutely* fixed. Therefore we should turn our attention to these concepts in order to explain their actual value and their psychological origin.

But postponing this study to the later chapters, we will first bring out some brief considerations concerning the physiological problems connected with logic, problems which result from the relation between its two aspects, the subjective and the objective.

C—THE PHYSIOLOGICAL ASPECT OF LOGIC.

§ 38. STATEMENT OF THE PROBLEM.

The two ways of regarding logic, as a system of laws regulating a psychological process, and as a system of conditions and hypotheses which give this process a value for scientific application, bring us face to face with a problem of the greatest importance.

How can we *explain* the relation between certain laws of thought and the world of phenomena, which is implied in the objective reality of logic?

The word "explain" here takes on a determinate sense if we regard the psychological process from the physiological point of view, thereby admitting that the phenomena of thought, the processes of association, and the laws by which these are governed, find their counterpart in a parallel order of physiological phenomena, in physiological processes, and in the conditions according to which the latter take place.

If we thus interpret the matter, the *thesis of empiricism,* which regards the laws of logic as generalizations from experience, is transformed into an *epigenetic* view of the development of physiological functions. The thesis of the critical philosophy, which recognizes in the logical laws the *a priori* conditions of thought, is changed into a *preformistic* view of the nervous structure, and especially of the cerebral structure; that is, of the elementary mechanical, physical and other conditions of the brain.

Now the first thesis, inasmuch as it concerns the individual, has been superseded because we have recognized how much the structure of a living being owes to heredity, and thus the object of research has been shifted from the individual to the species. The second thesis remains as a general view in this class of problems, and is too vague to be considered as an explanation of our difficulty.

Let us set aside those further biological questions which arise concerning the opposition of the epigenetic and the preformistic tendencies. What is clear to us is that the physiological structure of the organs of thought, as they *exist* in an adult, are such that the logical process corresponds to certain general facts. These conditions do not merely conform to the external facts, but rather they express the *manner in which the brain works.*

Now, however the brain itself may have acquired this *way of*

working, we ask whether it is possible to form an idea of the cerebral operations so as to bring to light those correspondences to which we have referred.

§ 39. FUNDAMENTAL HYPOTHESES.

To depict the physiological process of thought is such a problem that it seems over bold even to state it! What do we know of the most elementary phenomena that take place in the brain? A very few functions have been localized with some degree of certainty, by the anatomical method of degeneration, or by that embryology that is founded upon the growth of the nerve fibers.

On the other hand our knowledge about the elements of the nervous system does not go beyond a certain histological result which has lately been confirmed by perfected technical means. And it does not really seem probable that even if we advance in this direction we shall succeed in gaining a clearer view of the physiological functions of the nerve cells and fibers, unless we use arbitrary hypotheses.

The frequent changes in the current ideas as to the simplest questions, bear witness to the backward state of this class of studies.

To cite but one example, it is but a few years since the theory of the neuron seemed solidly established, and already the researches of Apaty have demolished its anatomical basis, while Bethe's experiment on the Carcinus Menas seems to break down its physiological foundations.

But we are not here attempting to represent those elementary physiological processes which are translated into thought, although we shall deal with some of their most general relations. And this investigation may be carried on in a similar sense to that of certain physical theories, in which we attain to a representation of phenomena by building upon a hypothetical mechanical substratum which is not in any way determined.

We may be allowed, for the sake of clearness, to choose certain hypotheses in the first place, from which we shall later free ourselves in some degree.

The *first hypothesis* is that the formation of ideas can be physiologically represented as a totality of *nervous currents,* in some fashion analogous to that which we can verify in the case of the lowest cerebral functions.

What the elementary phenomenon thus designated really is, whether in a certain group of cells or fibers there is produced an

electric discharge, or a series of the phenomena of metabolism; or whether osmosis or any other sort of chemical or physiological or physical phenomena, or quite another class of physical facts, these are questions which we may completely set aside.

Our scheme then represents a complicated interlacing of phenomena which, under the name of currents, we regard as binding together centers or groups of brain cells, according to action and reaction, transmitted by certain *paths of association.*

The *second hypothesis* (which we shall here state in its most restricted form) is that in the processes corresponding to a logical development, the currents may be regarded as distinctly *localized,* following paths which remain fixed, between certain groups of cells and fibers, during the course of the process.

This hypothesis may in a certain sense be justified by means of two considerations. The first is the principle that the "nerve current tends to pass again over the path already traversed." This principle, which does not imply any intimate knowledge of the nerve current, is an hypothesis generally accepted by neurologists, whatever may be their particular views as to the histological constitution of the tissues among which the phenomena occur.

In fact the hypothesis to which we have referred:

1. Forms the foundation of the usual genetic explanation of the reflexes.

2. It furnishes the guiding thread for researches into the localization of brain-functions, an idea which, however it may be enlarged and supplemented by the knowledge of the brain as one complete organ or unit, is still confirmed by a small number of experimental results, agreeing with the observation of certain clinical cases.

3. It amply explains the development of those vicarious functions which follow the destruction of some part of the cerebral cortex.

Now the physiological disposition of the nerve paths to become more accessible to the currents by which they have been repeatedly traversed *tends* to fix the paths of association, and so forms the most natural basis for the psychological law of association by habit, a law that has been well known ever since Descartes and Locke.[1]

But the manifold external stimuli, and the mutual reactions of the brain currents act in opposition to the tendency we have just

[1] William James, in his *Principles of Psychology,* says that modern science has not found the means of improving this old explanation.

been discussing. Therefore in an ideal representation that includes in a general way the whole series of cerebral processes represented as a system of currents, the paths of association cannot appear as distinctly localized.

But let us now refer to a second consideration, that is to the general fact of the *inhibition* exercised by the higher centers upon the lower. If in a given process an act of inhibition intervenes and isolates, so to speak, the currents from the perturbing action of neighboring currents, it would seem natural to suppose that these currents will remain localized within fixed paths so long as the inhibition lasts.

Our second fundamental hypothesis may then be considered justified, if we suppose that "the affirmation of an object of logical thought, or the assumption of a concept, corresponds to an act of inhibition which controls the corresponding physiological processes, in the way already stated."

The hypothesis of the localization of thought in the logical processes, may be understood in a wide sense. The easiest way to make this hypothesis plain consists of connecting the objects thought of with histologically distinct portions of brain tissue, as they are shown under the microscope, in groups of cells or fibers. But it is not really necessary to give the theory this precise anatomical significance. That which in the last analysis will prove useful in the following explanations is a certain invariability of those brain processes, which coalesce in a more complex process, while a logical association is in progress; and this invariability may even be understood in different senses.

§ 40. EXPLANATION OF THE AXIOMS.

Let us see how the hypotheses we have adopted concerning the mechanism of the logical process enable us to explain the relation between the laws of thought and the facts implied by the objective reality of logic.

The fundamental relation is that "the real properties of aggregates of objects, when the latter are under certain conditions of invariability, are expressed by those axioms which also formulate the laws of logical combination."

This relation is explained by the fact that logical combination corresponds, in our scheme, to the simultaneous formation of currents which follow constant paths, and hence, viewing thought phys-

iologically, "to combine logically" means to combine invariable brain processes.

According to this explanation, the verification of the axioms in a reasoning process could be experimentally ascertained by an observer capable of following the corresponding cerebral processes, and of perceiving that (at least up to a certain point) they are not modified by the fact that they take place simultaneously. An illogical reasoning process would result from a weakening of the powers of inhibition, a weakening whereby the processes in question would be made to interfere with one another, so as to vary the paths traversed by the currents. This fact could be verified by our supposed observer. This hypothesis finds its counterpart in the mental condition (already noted in § 26) of men who are wanting in logic, and who are incapable of abstracting systematically from the sentiments that accompany certain associations and tend to modify their product.

§ 41. CONCERNING THE IDEA OF CAUSE.

The same physiological hypotheses help us to explain the principal conditions of the idea of cause.

The mental state of *expectation,* which we express by supposing a relation amongst our sensations to be *invariable* when it *has not* varied, can be physiologically interpreted as a stimulation of those brain centers which are the seat of certain sensory images. This stimulation can take place, by virtue of established associations, whenever we reawaken the relevant sense images.

It is thus that we should imagine, from the physiological point of view, the representation of cause and effect.

And so in order that a constant succession of sensations may be represented as a causal connection, it is necessary that between the mental images of the antecedent and of the consequent there should be established a mental association.

In the causal conception which we designate by the word "*how,*" two clearly distinct images are found united. But on the contrary, in the causal conception corresponding to "*why*" we have a series of images associated by contiguity. In the latter case, thus differing from the former, the conception of the causal relation appears as a cerebral process sharply defined in its continuity, that is as a current or a series of currents, which establishes a connection between the image of the cause and that of the effect.

According to the principle that the nerve currents by their

passage determine the paths of association, we then have here not only the association between two *images,* but the *physiological condition which makes their association possible.* And this explains the feeling of necessity that accompanies this association.

These observations show the physiological difference between the two classes of causal conceptions, in the one of which the antecedent and the consequent are at the extremes of a continuous series, while in the other class of cases they remain sharply distinct. In case of the so-called *metaphysical explanations,* the continuous series of phenomena that join the cause to the effect is not really given, but simply assumed.

The psychological value of such an assumption is easily understood. It constitutes a *middle term* between the antecedent and the consequent that are to be joined in a causal relation. This middle term is an already well established series of associations by contiguity, to which our antecedent and consequent are attached as extremes.

Thus *in the tendency to metaphysical explanations considered from the physiological point of view we recognize the expression of a law of least effort, according to which the new associations representing causal relations tend to take place along those paths of association that have been already traced by other habitual processes.*

Such are the inductive conclusions of an analysis which, since I have stated it with considerable caution, ought not, I think, to be regarded as an over-bold or senseless research, even by those who are the least disposed to see in it even a step toward the solution of a great problem.

IV. GEOMETRY.

A—GEOMETRY AND REALITY.

§ 1. INTRODUCTION.

IT seems that we must give geometry a place of honor among philosophical studies!

In Greece it was a great power for intellectual training; and, together with rhetoric, it contributed toward building up the science of reasoning. And upon this very science the new critical geometry still throws, as we have seen, a bright light. Furthermore the movement of thought which accompanied the rise of our European civilization, began with a school of philosophical geometers. The rationalistic philosophy which, through its controversy with empiricism, trained the minds of men, and brought about the progress whence springs our modern thought—has come down to us from Descartes and received a new impulse also from Leibniz. And the two thinkers who exercised the greatest influence upon speculative thought in the nineteenth century, Emanuel Kant and Auguste Comte, owed their powers in great part to geometrical training.

But especially in the past century the progress of geometry has had a direct effect upon the development of a form of rationalism, which though far from the land of its birth, has attempted still loftier and more arduous flights, resulting in a really marvelous acquisition, the construction of the non-Euclidean geometry. In the light of this geometry it becomes evident that our geometrical notions, in so far as they refer to empirical reality, cannot at all lay claim to that rigid certainty, which used to be regarded as one of the strongest arguments in favor of their *a priori* character.

This view gives rise to new ideas and higher developments of geometrical philosophy, and in several ways tends to illuminate the physical problem of spatial structure. This insight has been aided

by the researches of Gauss, Lobatschewsky, Bolyai, Riemann, and Helmholtz, and by the more recent researches which we have sketched in our article on the "Principles of Geometry" cited on page 109—an article to which we shall also refer in the following sections.

§ 2. REALISM AND NOMINALISM.

In opposition to the developments to which I have referred, or at least in opposition to the interpretation of them that is commonly received by mathematicians, certain philosophers of the Kantian school still bring forward this objection: We cannot (they say) speak of geometry as a physical science, because space does not correspond to any real object, but merely expresses a subjective form of sensibility. And this nominalistic thesis reappears, in another form, in the more recent critical developments.

The controversy between realism and nominalism in geometry is one of the most delicate and important matters for philosophy in general. And the question is not so much to decide between two distinctly stated contradictory opinions, but rather, as often happens, to determine the sense in which both theses may be found valid, without contradiction.

By such a study it will then be made clear that the negative result, to which certain aspects of the problem of space lead us, does not preclude a legitimate sense in which geometrical realism may be recognized, while we also gain the positive result of bringing to light the facts that are comprised in geometry.

Thus to Kant's thesis denying the existence of a real object corresponding to the word "space," we, together with Herbart, shall oppose the view that "spatial relations" are real. And to the nominalism recently maintained by Poincaré, which declares that these relations do not possess a real significance *absolutely independent of bodies*, we oppose a more precise estimate of the sense in which geometry is a *part of physics*.

§ 3. SPACE AND SPATIAL.

Let us ask the question "what is space?" and try to furnish an adequate critical answer.

Let us consider any sort of body, as for example a piece of copper, or iron etc., which is immersed in air, or water, or any other fluid. The notion of this body permits us to distinguish certain sensations which refer to the matter *inside* or *outside* of it.

Numberless cases, differing as to the material constitution of the body or of the medium containing it, still possess some common features, by means of which, *abstracting from the perceptible peculiarities* that we associate with copper or iron, with air, water etc., we gain the notion of a "*special way of separating matter from matter.*" This is the objective content of the geometrical concept of *solid* and of *surface.*

Let us take for example a ball that is capable of growing constantly larger. We say that when it shall have *become infinite,* it will have filled all *space.*

It is no wonder that this transcendental procedure leads us to attribute to the word "space" an entirely illusory meaning! In fact, because the very notion of a sphere implies a way of distinguishing the sensations that pertain to its interior and those that pertain to its exterior, an infinite sphere, which cannot correspond to any separation of this sort, no longer has any real significance. And so we see how space, defined in such a way, is but an empty name devoid of meaning.

Need we prolong such a negative critique? We are inevitably reminded of the reasoning of Don Ferrante in Manzoni's "Promessi Sposi," by which he proves that the contagion of the plague can be neither substance nor accident.

The same analysis, further developed, also shows however that, apart from the transcendental sense of the word, there remains an actual physical significance belonging to the *spatial relations or to the positions of bodies,* whose totality may well be denoted by the word "space," taken in a positive sense. Now since the relations of alignment, equidistance etc. correspond to a fixed agreement between certain voluntary acts and the sensations following them, it follows that the space relations which we have mentioned give us a knowledge of reality. In fact, nothing can be more fixed and precise than the previsions of geometry!

§ 4. A CRITIQUE OF SPACE RELATIONS.

The acceptation of the word "space" as signifying an infinitely large body, is not the only one that is based upon a transcendental process of definition, and that therefore leads to a nominalistic conclusion. The space relations also can be understood transcendentally, either by attributing an *absolute* sense to their *generality,* or by granting to them an *infinite exactitude,*—two modes of interpretation which appear, for the rest, to be bound together.

The universality of geometry is due to the fact that distinctions of space *do not depend* upon the matter that is distinguished,—as for example the outside or the inside of a given sphere. But this independence only signifies the coexistence, or the possibility, of such distinctions referring equally to *different substances*; and does not signify an *absolutely general* physical relation *peculiar to space in itself,* such as might have a meaning *apart from all matter.*

The attempt to give geometry any significance apart from bodies, is bound up with the attempt to seek an infinite or absolute exactitude in geometrical relations. Yet, if this interpretation of the universality of geometry is rejected, the exactitude that belongs to mathematical doctrine cannot be immediately applied to the physical world. For we observe that no real object corresponds to the mathematical concepts "point," "line," "surface," "straight line," "plane," "distance," etc.

To the obvious fact of the non-existence of such objects, we may also add that, in physical reality, it is not even given to us, beyond a certain point, to verify the presence of anything corresponding to any of these geometrical figures, even if it existed. We find, first of all, a limit belonging to our senses, a limit which indeed may be removed by the aid of suitable instruments. But we further meet with a new limit in the imperfection of these instruments, and that not only from the practical, but also from the theoretical point of view. So, for example, the length of a light-wave forms a theoretically impassable limit to the precision of sight, however aided by the microscope.

§ 5. THE NEW NOMINALISM OF H. POINCARE.

The impossibility of attributing to the space relations of bodies any sense apart from the bodies themselves, and the non-existence of any real objects represented by the mathematical concepts "point," "line," etc.,—these facts are interpreted by H. Poincaré[1] as a decisive refutation of geometrical realism.

Although in the work just cited we do not find a preliminary criticism of what "space" may signify, the transcendental sense which the author attributes to geometrical relations comes out clearly in some passages of his work which we will now quote.

"Experience only teaches us the mutual relations of bodies. No experience deals with or can deal with the relations of bodies

[1] Cf. *Science et hypothèse,* Flammarion, undated.

with space, or the mutual relations of parts of space." (*Op. cit.*, pag. 100).

"Do you say that if experience deals with bodies, it must at least deal with the geometric properties of bodies?

"And now, what do you understand by the geometric properties of bodies? I suppose you mean the relations of bodies with space...." (*Op. cit.*, p. 101.)

"....there exists no property which can....be an absolute criterion whereby we can recognize a straight line and distinguish it from every other line.

"Should we say for example,—this property is as follows: A straight line is a line such that a figure of which it forms a part may be moved without a variation in the mutual distances between its points, and in such a way that all the points upon this line will remain fixed?—

"Here we have indeed a property which....belongs to the straight line and to it only. But how can we tell by experience whether it belongs to such and such a concrete object? We must measure the distances, and how can we know that a certain concrete dimension, that has been measured with a material instrument, really represents distance in the abstract?" (pp. 94-95).

Now such observations only serve to confirm the conclusion already stated, which is moreover evident: Pure geometrical relations are an abstraction to which no objective reality corresponds. The elementary concepts of geometry, as "point," "straight line" etc., are also abstractions which have no exact counterpart in any object. They serve then as *symbols* to express certain relations of position amongst bodies, relations which are stated by means of the propositions of geometry.

If Poincaré limited his analysis to this, he would indeed be irrefutable. But our distinguished author has gone still further. He has concluded that these propositions of geometry do not apply to any real fact, but should be regarded as a pure *system of conventions* by means of which we can express physical facts, just as we refer size to a system of measures. This system, as he teaches, may be convenient; but there is nothing to hinder us from changing it. To ask whether a phenomenon is possible according to a certain geometrical hypothesis, and impossible according to the opposite hypothesis, would be like asking whether there are lengths expressible in meters, but not in English feet (cf. *op. cit.*, p. 93).

But this conclusion seems unacceptable. The fact that the prop-

ositions of geometry are theoretically expressed by means of the relations between concepts, which in mathematics must be regarded as symbols, does not suffice to rank them as purely arbitrary conventions with respect to the physical world. For in that world these symbols find an approximate correspondence in certain objects for which they stand.

Poincaré has indeed tried to confirm his opinions by means of certain charming artistic constructions. He has tried to imagine physical conditions in which *the same space* in which we live *shall appear to be endowed with different properties* from those of our geometry. And for this task it suffices to suppose that moving bodies change their form according to certain laws, for example, by the action of a change of temperature *which depends upon their position*; that light does not travel in a straight line, when subjected to the action of a refracting medium *suitably distributed in the field of space* etc. (*op. cit.,* p. 84 *passim*).

But a thorough study of these examples will show that these hypotheses, when interpreted with regard to the relativity of our knowledge, imply a *real* change of space, or of the relations denoted by that name.

In our world bodies can be measured, each one in comparison with others, because we are able to move them independently of any change in their physical condition. Heating, cooling and pressure, it is true, modify the objects compared when we measure; but these modifications are accidental in relation to the reciprocal position of the bodies themselves, and therefore geometry need not take account of them.

In the world imagined by Poincaré, on the contrary, temperature would be a true *geometrical character,* since all bodies (including our organisms) would have the temperature belonging to the place occupied by them. It would be no longer possible to bring into contact, and so to compare, the dimensions of bodies variously heated. And so we should no longer be able to say that "bodies expand with the increase of their temperature." Therefore, in this other world, geometry would be *really* and not merely apparently different from ours.

The contrary statement either takes the contrast of reality and appearance as something transcendental; or else it contradicts the the very data supposed. For it surreptitiously introduces into that world, a judgment made after *our* image, which is not subject to the laws of such a world!

We could undertake a similar critique of the hypothesis that light does not travel in a straight line. Nothing is easier, from the abstract point of view, than to accept such an hypothesis! Is it not actually realized in any heterogeneous medium, according to the laws of refraction? But the heterogeneous nature of the medium, which can be verified by the proper physical experiments, is still an accidental element of the phenomenon. The paths of the light-rays can in this case be *changed*, by altering the arrangement of the medium itself in the field of our experience. On the other hand, what would be the meaning of a heterogeneous medium, with a fixed distribution in space, for example an ether of varying constitution, whose parts do not change with regard to each other with the movement of the bodies? Should we not be creating, with such an hypothesis a *geometrical world* different from ours? For our geometrical world is *homogeneous*.

In studying such questions it will help us to take account of the physical significance of the "straight line."

The concept of the straight line is derived from the study of various orders of phenomena:

1. From that of the movements of *solid* bodies, in which the straight line appears as an *axis* whose points remain motionless during a rotation (hence like a stretched cord etc.).

2. From the dynamics of *particles*, in which the straight line appears as the *path* of a point whose motion is not modified by changes in the surrounding bodies.

3. From optics, and in general from the study of radiations, in which the straight line appears as a ray or as a line of symmetry for the phenomena, in case the medium can be viewed, in the light of definite experiences, as *homogeneous*.

The first and third properties of the straight line chiefly serve to define it with respect to the senses of touch and sight, while the second might perhaps serve as the basis of a definition in relation to the muscular sense.

Now the one as well as the other of these definitions of a "straight line" will permit the foundation of a system of geometry. To particularize, we can have a (*metric*) *geometry of solid bodies*, and an *optical* (*or projective*) *geometry*.

The agreement between the various ways of regarding a straight line, as an axis and as a ray, is a fundamental fact which enables us to include the two classes of phenomena in a *single geometrical representation*. Other known phenomena, and especially dynamic

phenomena, can also be grouped together with these classes of phenomena.

The foundation of this geometrical representation is that certain conditions of homogeneity in the medium imply a *symmetry of the phenomena* with respect to certain lines. Such a relation of dependence constitutes in its true physical sense the *homogeneity of space.*

Nothing hinders us from imagining that the manifold agreements whose totality is expressed in the symmetry to which we have referred, would no longer really exist for us if we studied the facts by means of *more exact* experiments than are now possible. But in that case, it would be quite a different matter from simply rectifying a proposition of the theory of light, for the reason that the sum total of the facts that are implied by the *hypothesis of the straight line* would no longer be true for that advanced order of approximation.

§ 6. GEOMETRY AS A PART OF PHYSICS.

Those philosophers who, attributing a transcendental meaning to geometry, are led to nominalism, seem to be the victims of a fundamental illusion which comes from regarding our knowledge solely in its *completed,* that is, in its *actual* aspect, without taking account of its genesis.

This is precisely the view of science that Kant took from the study of Newton's system. The kingdom of theoretical science, in which the more complex relations appear in subordination to the simpler, was interpreted by him in an absolute sense.

Thus in particular, the fact that in a dogmatic treatise geometry *precedes* mechanics and physics, and that the facts and principles of geometry stand at the base of the very experimental method that is used in physics,—this fact is changed into a relation of necessary dependence which finds expression in Kant's "*a priori.*" And, passing from the criticism of scientific knowledge to that of ordinary knowledge, the intuition of space relations, which regulates the associative processes of our actual sensations (sight and touch),—this intuition is regarded as *a priori* in relation to these sensations. Separately from the data of the senses (fictitiously taken as simple elements and isolated in space and time), spatial order is conceived as a *scheme* added to them by the mind, in which they find their place.

Now this whole epistemological construction has been superseded by a more adequate view of the development of knowledge, a view which constitutes the surest gain of the philosophy of evolution.

The general fact that experience is interpreted by means of previous knowledge, and that every phase of scientific progress is, in a similar way, dependent upon a preceding phase, is to-day taken in a sense different from that of Kant. We no longer think that the general fact just referred to establishes an absolute hierarchy among the sciences; but rather, even in this very fact, we perceive the gradual development of each science. In this development certain notions that are simpler or more precise stand out from the mass of crude empirical data, so as to form a relatively autonomous body of doctrine.

Therefore geometry is not only viewed as a necessary introduction to physics, but comes to be considered as a part of physics, a part which has attained to a high degree of perfection by virtue of the simplicity, generality and relative independence of the relations included in it.

To be sure when the propositions of geometry are taken in a physical sense, the concrete previsions contained in them are bound up with certain elements of fact that we commonly consider as non-geometrical. Therefore the theorems of theoretical geometry appear merely as the symbolic expression of physical relations. In geometry these relations are incompletely stated. They receive precise determination in their concrete applications.

With respect to the latter, the precise form of the theorems represents only a grade of approximation, which can be carried further in case we convert the *equalities* into *inequalities,* as may be seen in the following example:

Let us examine the theorem "the angles at the base of an isosceles triangle are equal." In reality certain objects exist (physical triangles) which to some degree of approximation can be represented by the concept "triangle." Let us construct such a model with three thin steel rods, or by drawing a diagram on paper. Let us measure two of the sides with a rule, and the two opposite angles with a goniometer.

The real measure of a side is represented by two numbers, whose difference (perhaps m. 0.0001) depends upon the perfection of the instrument. This can be expressed by saying that the length of the side is equal for example to m. 3.4576, with a possible error

of less than a tenth of a millimeter; that is to say, the length is somewhere between m. 3.4576 and m. 3.4577.

If the two sides of the triangle are measured by the same number 3.4576 in the sense just defined, the triangle is considered to be "isosceles." Our premise then is that "the two sides are equal within m. 0.0001."

Now the measure of the two angles of the triangle, opposite to the sides in question, is given by the goniometer. The theorem just stated tells us that the difference between the two angles will be very small, so that they will appear equal, to a degree of approximation dependent upon the accuracy of the measurement of the sides.

But this is only a vague indication. If we wish to find the precise meaning of the theorem as a statement of fact, we must transform it in the following way:

"If two sides of a triangle differ by less than a certain length ϵ, the two opposite angles will differ by less than a quantity τ, which depends upon ϵ according to a certain law." And hence we must complete the theorem itself by finding a function $f(\epsilon)$ such that for an ϵ less than a certain limit, $\tau < f(\epsilon)$.*

By a simple calculation we find (τ being represented in degrees)

$$\tau < 61.\epsilon/a,$$

in which a expresses, with a possible error of ϵ, the length of the apparently equal sides of our triangle. In the present case

$$(\epsilon < 0.0001, a > 3)$$

and hence we have

$$\tau < 1''.$$

When the theorems of geometry are converted from equalities to inequalities in the sense just illustrated, we perceive that they represent part of the relations of position amongst bodies. What we need to add to these relations, in the various concrete applications, belongs simply to the nature of these bodies themselves (to their heat, to the forces acting upon them etc.), and is considered to be foreign to theoretical geometry.

Although the distinction between theory and application is only conventionally introduced to simplify our view of reality, such a simplification is made possible by the existence of a *statistical regu-*

* F. Klein, in his excellent lectures in the summer of 1901 has called attention to this class of questions which belong to geometry when viewed as a physical science. (*Anwendung der Differential- und Integral-Rechnung auf Geometrie.* Leipsic, Teubner, 1902).

larity that is superimposed upon the *irregularity of phenomena.* And this simplification may be interpreted by the assumption of general and precise geometrical facts referring to hypothetically simplified conditions.

§ 7. ON THE EXACTITUDE OF GEOMETRY.

Let us try to explain this idea, in order that we may the better appreciate the exactitude of geometry.

The *direct* verification of a geometrical property cannot, in any single case, surpass a certain limit of approximation. This limit may be pointed out *a priori* in relation to the limit that we have already found to hold for the construction of our geometrical objects themselves. But if that geometrical property is regarded as an hypothesis relating to theoretically simplified conditions, the verification can then be supplemented by so many indirect sorts of proof that we can no longer set any *a priori* limit to its exactitude.

In this sense geometry appears as a system of *general hypotheses* whose significance lies in the *totality of the facts* derived from them. And this therefore is connected with a series of experiences which can be extended without limit. In this same way we have already interpreted the existence of the straight line as the assumption of a general symmetry amongst certain phenomena. With this symmetry we can connect verification of the properties of the straight line that are far more precise than those immediately furnished by the properties of the axes on which solid bodies rotate.

It is important to emphasize this point: These indirect verifications founded upon the assumption of a geometrical relation, *have a meaning* notwithstanding the impossibility of realizing (in the requisite degree of approximation) those simple conditions to which the relation itself refers. This comes out with especial clearness through the statistical considerations to which we have already referred. To deny a geometrical hypothesis is tantamount to admitting a *systematic* cause of error. If such a cause of error were added to the various physical conditions, it would become manifest in the long run as something distinct from accidental irregularities.

Thus, to go back to one of our previous examples, the imperfection of our models and the inexactitude of our measurements may well show an error in the verification of the theorem "the angles at the base of an isosceles triangle are equal." But in a large number of figures based upon this theorem, these accidental errors tend to counteract each other. Thus it appears that in this sense

the inaccuracy of our *application* does not vitiate the accuracy of our *theorem.*

Let us try, on the other hand, to make repeatedly, in a series of drawings, an approximate construction, for example, that of Specht for the rectification of the circumference of a circle. In such a case a systematic cause of error is introduced, which will become constantly *more perceptible,* in the course of a large number of observations.

Now in this way the *exactitude of geometry* comes to have a precise significance: At every moment during any scientific progress, there is an hypothesis that is verified *up to a certain point,* by experiment, and which anticipates the results of other possible experiments. And it is clear that this hypothesis can never be finally proven, because the series of possible experiments is unlimited. There is no reason however why it may not be *disproved* if untrue.

In any case we cannot well define the doubt that here arises, unless we try to imagine an opposite hypothesis, which shall be as definite and as coherent as ordinary geometry.

This is precisely what has come to pass in the construction of the non-Euclidean geometry, which shows that, without contradicting the facts upon which the definitions of the fundamental entities of geometry are based, we can make hypotheses that are irreconcilable with the exactitude of ordinary geometry, and which are expressed instead by *a true and proper geometry that is different from ours.*

§ 8. SPACE AS A CONCEPT: ABSTRACT GEOMETRY.

In order to avoid misunderstandings let us begin with the following observations:

Space, as a geometer regards it, is not so much an intuition according to which we arrange our sense-images, as a *concept.* This the geometers have constantly affirmed ever since Leibniz.

The Kantian critical philosophy has made the mistake of failing to recognize this psychological meaning of the word "space," a meaning that geometry cannot dispense with, if it is to take on the logical form of a deductive science.

The objection is vain that a concept expresses properties common to various objects while space is unique.

Those who still believe that this old argument is of some value, seem not to know of the abstract logical significance that space has acquired for the geometers of to-day.

The concept of space, in its mathematical acceptation, represents the sum total of the (geometrical) relations that exist among *points,* considered apart from the particular sensations connected with the image of a point. Space is thus conceived as a *manifold of any elements whatever,* to which we give the *name* "points" simply because they occur in certain ordered relations that are fitted to represent with considerable approximation the relations of place existing among very minute bodies (physical points).

We have already found occasion to notice how Plücker has taught us to profit by the generality that belongs to the concept of "a point." For according to him, under the name of space, we can study a great variety of geometrical forms.

We may give, for example, the name of "points" to the circles in a given plane, and we may define according to our convenience what shall be understood by the "distance" of two circles and what systems of circles we will call by the names "straight lines" and "planes." These definitions ought to be so stated that the elementary geometrical properties relating to ordinary space (the postulates) can be expressed in propositions that are valid for the geometry of these circles.

When these conditions are fulfilled, we have established a correspondence between the ordinary space composed of points and this conventional space composed of circles. Every figure composed of points has for its counterpart a *figure composed of circles,* and every proposition concerning the former figure may be translated according to this *duality,* as a proposition concerning the latter.

When we follow Plücker's principle into the realm of geometrical applications, it seems natural to compare with physical space, some other varieties of elements in which the properties expressed by the translation of the postulates of geometry are only partly satisfied.

Thus a series of *abstract spaces* arises, for which *different geometries* are valid. They must however be constructed on a common basis.

For example, we can conceive of a series of non-Euclidean spaces, in which the postulate about the parallels is not satisfied, and this series, which depends upon the value of a certain constant, improperly called a *curvature,* now falls under the concept of a space generalized from the concept of ordinary space.

By a further generalization, we can make this concept include

the series of *spaces of many dimensions,* or even other kinds of space still more distant from physical space.

There is no necessary limit to this process of generalization, which depends solely upon a *convention* that the mathematicians have found it useful to establish in the interest of their studies.

The theory of abstract spaces is really an essential part of the geometrical structure which has been built up during the past century! Without considering what has thus been learned in the field of mathematical analysis, let us dwell a little upon the interest which this theory may have for the philosopher. This interest is twofold, since it may refer to the question of physical space, or to the origin and development of our knowledge of geometry.

§ 9. HISTORICAL HINTS REGARDING THE NON-EUCLIDEAN GEOMETRY.

From the time of Euclid up to the beginning of the past century, it was universally admitted that we have, in the concept of space, as defined by the axioms and postulates of Greek geometry, a rigidly exact representation of the physical relations of position.

To doubt the accuracy of this representation seemed mere folly to every one so long as the idea of space in question was conceived as being the only one *possible.* Therefore it is not too bold to suppose that even the greatest genius would have entirely failed in this arduous critical task, had he not been led in that direction by some defect in the logical structure of the Euclidean geometry.

Since it was then believed that the fundamental propositions of geometry must have the character of axioms, and that they are necessary in the same sense as the axioms of logic, and are of much the same nature, people found it difficult to make up their minds to add to these the somewhat less evident proposition that forms Euclid's fifth postulate about parallels.

At this point it will be useful to remember this postulate, and the content of the premises upon which the Euclidean geometry rests.

According to Euclid, geometry is founded on definitions, common notions, and postulates. The purpose of the latter, which assert the possibility of certain elementary constructions, involves affirming the *existence* of the fundamental entities of geometry. For this was the very office which the Greeks attributed to geometrical constructions. (Zeuthen.)

But there are actually other hypotheses, sharply distinct from

the logical axioms, which are implicitly contained both in the definitions and in the common notions of Euclid. And modern critical study, while regarding the fundamental geometrical concepts as undefined, calls these other hypotheses also by the name postulates (chap. III).

There are various ways of stating and arranging the premises that are used in the Euclidean geometry.[1]

It may here be permissible to use the plan which lies at the basis of our *Questioni riguardanti la geometria elementare.*[2] This plan has been carried out, in accordance with our own pedagogical ideas, in the *Elementi di geometria*[3] which we edited together with U. Amaldi. For in this plan we have explicitly pointed out exactly what, in Euclid's assumptions, precedes the hypothesis about the parallels.

Let us then enumerate the premises which deal with:

1. The postulates relating to the determination of straight lines and planes and their mutual interrelations.

2. The fundamental properties of the *straight line,* viewed as a system of points (order systems, segments etc.)[4]

3. The properties of a *plane surface* with respect to straight lines (division into parts, angles).

4. The division of space into parts with respect to a certain plane.

5. The possibility of *movements,* that is to say the principles of the *congruence* of figures (from which follow the theorems about congruent or equal triangles etc.).

6. The postulate of the parallel lines (post. V in Euclid):

"If two straight lines in a plane are cut by a third in such a way that the sum of the internal angles formed by the transversal and lying on one side of it is less than two right angles, the two straight lines, if prolonged, will meet on the side in question."

Numerous attempts to demonstrate this postulate (for which we can substitute the assertion of the uniqueness of the parallel through a point to a given straight line etc.) have been made, beginning with the first commentators on Euclid (Proclus, Nasir Eddin...)

[1] Cf. our article on the "Principii" already quoted.

[2] Bologna, Zanichelli, 1900.

[3] Bologna, Zanichelli, 1903; 2d ed. 1905.

[4] Amongst these properties we find the *continuity* of the straight line. This does not appear in Euclid, although upon occasion we find particular consequences of continuity instead of the general principle. Compare article IV by G. Vitali in the collected articles of F. Enriques "Questioni...." *op. cit.*

and continuing up to Legendre. But these proposed demonstrations, either explicitly or less obviously, have recourse to some other proposition, equivalent to postulate V in Euclid, and not contained among the collection of geometric properties founded upon the hypotheses 1....5.

Without going fully into the history of the attempts referred to, it is enough to bear in mind a few names and dates that are the most closely connected with the growth of the non-Euclidean geometry.[1]

John Wallis (1663) showed that the postulate of the parallels is a necessary foundation of the theory of similar figures, so that by admitting the existence of similar triangles Euclid's postulate can be proved.

Girolamo Saccheri, in his *Euclides ab omni naevo vindicatus ....*" (1733), starts with the construction of a quadrilateral having three right angles, and distinguishes the three hypotheses which (in relation to properties 1....5) appear *a priori* as possible concerning the fourth angle: namely that it might be right, obtuse or acute. He shows that each of these hypotheses will be found to be verified for *every* quadrilateral, under the above conditions, in case it is verified by a single quadrilateral.

Hence he establishes, in the three cases, the fundamental property of two straight lines in a plane perpendicular to a third:

In case of the 1st hypothesis they will be everywhere equidistant (from which follows Euclid's postulate of the parallel lines).

In the second hypothesis, starting from the common perpendicular, they will continue to *approach each other.*

In the 3d hypothesis, on the contrary, *they will diverge.*

The hypothesis of the obtuse angle (2d) is hence eliminated by Saccheri because it is inconsistent with the *infinity of the straight line.* As to the hypothesis of the acute angle, the author used fallacious arguments. But he was convinced *a priori* that such an hypothesis must turn out to be impossible!

This error with which Saccheri's work closes, does not destroy the value of those of his results which we have already mentioned. Another of his results is the following:

If the hypothesis of the obtuse angle is valid, the sum of the angles of any triangle whatever is always greater than two right angles. According to the hypothesis of the acute angle, it is on

[1] For the history of this newer geometry, and also for a clear development of its theories, see article VI by R. Bonola in our collection already cited.

the contrary less than two right angles. If for one triangle the sum of the angles is equal to two right angles, the same will be true of any other triangle, and so the Euclidean hypothesis will hold good.

The latter conclusion was rediscovered, a century later, by Legendre, at the end of his lengthy study of the theory of parallel lines.

J. H. Lambert, in his *Theorie der Parallellinien,* published in 1786, but written, it seems, twenty years earlier, discusses afresh the three hypotheses of Saccheri, whose work, as Segre plausibly suggests, may have been known to him. He concludes that the hypothesis of the acute angle cannot be so easily refuted as the other, which is inconsistent with the infinity of the straight line.

We pass over the work of F. C. Schweikart and F. A. Taurinus who have been shown by Engel and Staeckel's historical researches to be amongst the nearest forerunners of the non-Euclidean geometry. Hereupon we may say that the latter assumes definite form in the researches of Gauss, Lobatschewsky and Bolyai.

The studies of Gauss, which were begun between 1792 and 1797, continued up to 1832, and are known to us only through the great geometer's letters.

The work of Lobatschewsky is contained in the publications of the Courier of Kazan beginning in 1826, while his Hungarian confrere's earliest publication on the subject bears the date 1829.

Lobatschewsky and Bolyai developed to the full the consequences of Saccheri's third hypothesis, which states that two straight lines in a plane perpendicular to a third will continue to diverge. These consequences form a coherent body of *abstract* geometrical doctrine which Gauss called "non-Euclidean geometry."

From such developments comes the proof of the *impossibility of demonstrating Euclid's postulate about parallel lines,* by deducing it from the premises indicated above. This proof is implicitly given by the formulae of the non-Euclidean trigonometry, as set forth by Lobatschewsky and Bolyai. However, these geometers have not succeeded in making the proof so explicit, as to exclude the possibility that in pursuing the study of non-Euclidean geometry, one may still run against a contradiction. And Gauss himself, who on his own account had long since reached the principal theorems of this geometry, seems only to have become convinced of its logical possibility towards 1830.

These details have to-day only an historical interest. The

logical possibility of a non-Euclidean geometry, and hence the undemonstrability (in the sense pointed out) of the ordinary postulate about the parallel lines, is henceforth beyond question.

And, to go still further, it has been seen that *yet another* geometry is possible, in which one takes away from the premises 1....5 the hypothesis of the infinity of the straight line, (regarding the straight line as a *closed line*). We then have a body of abstract geometrical doctrine, which takes the name of Riemann, and which is as coherent as Lobatschewsky's geometry. While the latter corresponds to the development of the hypothesis of the acute angle considered by Saccheri, Riemann's geometry corresponds to the hypothesis of the obtuse angle.

But we cannot continue the account of the further discoveries accomplished by mathematical science in this class of ideas. And so we must now omit the account of the contributions which have been brought to these researches by such geometers as Helmholtz, Beltrami, Cayley, Klein, Clifford, Lie, Poincaré, Veronese and Hilbert.[1]

We shall later refer to some of the discoveries of these mathematicians. We will now follow the founders of the non-Euclidean geometry into those philosophical consequences that have been suggested by their discoveries.

§ 10. THE PROBLEM OF SPACE.

It is a peculiar characteristic of the human mind that it is impelled to seek in the world of reality the concrete models of its own creations. This disposition passes unnoticed, as it were, in those minds that do not deal with abstract studies, because the concepts which they form, resulting from the proximate associations of the data provided by the senses, appear from the very beginning, precisely as if they were the representations of real objects. However, even in these cases, the procedure which extends the results of experience is a procedure of abstraction, resulting in a concept to which new observations tend to be assimilated.

But in the case of minds that have been trained in the abstract sciences, the process of constructing concepts goes immeasurably beyond the proximate association of the sense data, and thus the resulting ideal creation appears as entirely apart from reality. It is precisely for this reason that the attempt to give a certain objectivity to the abstract strikes us as a peculiar quality of this class of minds. If any one believes this to be a blameworthy defect of

[1] Cf. our article on the "Principii" already cited.

mathematical minds, we can only say that he has not looked below the surface of the question.

For indeed, the failing of the mathematical mind, in its lesser representatives, is exactly the reverse. That is, the failure to comprehend that a thought which makes use of abstract constructions, without the hope, however vague, of finding therein the scheme of some reality, would be but a sterile instrument of dialectics.

"To perceive in every abstract concept, however defined by a constructive logical process, a *possible* representation of reality," this is the guiding view of those who join with the faculty of abstraction, a clear consciousness of the scientific purposes for which abstraction is of use.

Nor should we blame one who seeks this *possible* reality by means of those ideal constructions which are so distant from the data of immediate observation. We might as well blame a genius for the daring hypotheses wherewith he tries to see beyond the clouds that limit the common horizon. This daring is only blameworthy when the possible hypothesis is accepted as true, or as probable, without sufficient verification, or whenever his enthusiasm blinds the student to those facts that are contrary to his hypothesis.

The founders of the non-Euclidean geometry, in granting to their abstract developments the value of an hypothesis about reality, were certainly bold, but as we shall see, they do not deserve the blame to which we have referred. They were at once mathematicians and philosophers when they propounded a question that marks the greatest victory of the critical mind; and, as philosophers, they were positivists in the best sense, because they sought the solution of their doubts in the facts, and they weighed these facts calmly.

Kant had labored to prove the subjective character of our space intuitions, by seeking to disprove their objective significance. But Gauss turned his attention to the more precise measurement of the angles of the geodetic triangle whose vertices were Brocken, Hohehagen, Inselberg[1] in order to obtain thus the experimental demonstration of geometry in that degree of approximation that is accessible on our earth. And Lobatschewsky investigated the parallaxes of the distant stars, to ascertain whether, in such a class of measurements, the ordinary theory of parallel lines could be regarded as valid, or whether it ought to be replaced by the non-Euclidean

[1] Gauss mentions this triangle in § 28 of his *Disquisitiones circa superficies curvas.* Its sides are about 69, 85 and 197 km.

theory. A similar view had already occurred to Schweikart, and he had given this geometry the name "astral."

Let us try to estimate the value of such experiments. In view of what we have said, the question of the parallel lines may be decided *theoretically*, by inspecting a single triangle. If the sum of its angles is equal to two right angles, Euclid's hypothesis holds good. If it is less, we must accept Lobatschewsky's hypothesis, and if it is greater, that of Riemann. In the last two cases, if we let α equal the difference between the sum of the angles and two right angles (which in Riemann's case is positive) and let A equal the area of the triangle, we can then get these two equations respectively:

$$\alpha/A = -4/\pi k^2,$$

or

$$\alpha/A = +4/\pi k^2.$$

And hence it is demonstrated that k has a value independent of the particular triangle under consideration.

The constant α/A is called the *curvature* of space (because of certain analogies with the theory of surfaces). It reduces to 0 in the Euclidean hypothesis, which is obtained as the limit of the preceding for $k = \infty$.

It therefore follows that the *physical* solution of the question of the parallel lines can only be obtained if, by accurate measurements, we prove the sum of the angles of a triangle to be *perceptibly* less or else greater than two right angles. But if that sum is found to be perceptibly equal to two right angles, there remains a doubt between two hypotheses:

a. The Euclidean geometry is physically valid for any exact measures we choose.

b. One of the two non-Euclidean hypotheses is valid, but the (negative or positive) curvature of space is very small, and hence Lobatschewsky's parameter k is very big; that is, so big that the sum of the angles of our triangle differs from two right angles by less than the errors of observation.

Now precisely the most accurate measurements of triangles on the earth give, as Gauss observed, a perceptible verification of the Euclidean geometry, allowing for the errors of observation. It follows that k surpasses a certain limit with respect to terrestrial dimensions.

Let us turn to astronomy. Here we meet with triangles whose dimensions are enormous in comparison with those that can be ob-

served on the earth. The denominator of the fraction $4/\pi k^2$ being much greater, we may hope that the quantity α will be appreciable, even if k surpasses the limit indicated above. Meanwhile we cannot measure the *three* angles of a celestial triangle whose sum is supposed to differ by α from two right angles, but we can only measure *two* of these angles. Thus for example, if we look at a star from two opposite points of the earth's orbit, namely the extremes of the major axis of this orbit, we cannot measure the angle x which this axis, seen from the star would subtend, but only the two angles a, b, which are formed with this by the visual rays going to the star.

According to the Euclidean hypothesis we have

$$a + b + x = 2\mathrm{R},$$

and hence the angle x is given by the *parallax* of the star, $2\mathrm{R} - (a+b)$, which is usually determined by reducing it to the case in which one of the two angles a, b, is a right angle. The more distant the observed star is, the smaller does its parallax become, although it always remains positive.

On the other hand, according to the hypothesis of Lobatschewsky, the parallaxes of all the stars would be above a certain limit (depending on k). On the contrary according to Riemann's hypothesis the parallaxes of the very distant stars would have to be negative.

Let us note this: If the parallaxes of the observed stars were all found to be positive, and above a certain limit, we could not really conclude that the Euclidean hypothesis of the parallel lines was disproved. The more natural supposition would rather be to admit that the distances of the stars in question were all below a certain limit. In any case the way would not be closed to new and more accurate studies of terrestrial triangles, or of the mechanics of the planetary system, from which the experimental proof of Lobatschewsky's geometry *might* result.

If on the contrary the parallaxes of some stars were shown to be negative, then, so long as we hold fast by the hypothesis that light travels in a straight line, the validity of Riemann's geometry would be proven.

But *observation teaches us that the parallaxes of the stars are all positive or not perceptible at all.* And this is the result we obtain from the *nearest* stars, that is to say from those that have a parallax appreciable with our instruments.

For the parallax of 1″ Lobatschewsky computes that its param-

eter k is two hundred thousand times as great as the major axis of the earth's orbit, which measures about three hundred million kilometers. But there are stars whose parallax is $< 0''.1$, so that k is two million times greater than that axis. And most of the stars are still more distant, so that they give no appreciable parallax!

And so physical space may be considered as Euclidean, to a degree of approximation that actually surpasses the measurements furnished by our most highly perfected instruments. This is Lobatschewsky's conclusion, which may be thus expressed: *In the present state of our knowledge, physical space must positively be regarded as Euclidean.*

But this does not justify the assertion that the matter *could not* be otherwise. And it is unjust to accuse the non-Euclidean geometers of having raised a doubt, which is only removed for the present, and perhaps postponed to a distant future. For if the parallaxes of the stars had all been found to be above a certain limit, it is not to be believed that we should have been satisfied with the supposition that their distances were all below a certain quantity. At least Lobatschewsky's geometrical hypothesis could assume in that case a more concrete form, in relation to not *too* great a value of the parameter k. It would therefore become possible to test this hypothesis in various ways, as we have already pointed out, either by increasing the precision of terrestrial measurements and multiplying their number, or by studying the mechanical consequences of the hypothesis with respect to the planetary system. And it might well happen that certain small divergences from Newton's law might thus find a sufficient explanation by bringing to pass a greater correspondence of the results.

With still better reason, if the parallax of certain stars had proved to be negative, we should have had a solid foundation for adopting the non-Euclidean hypothesis, as later formulated by Riemann.

Nor is it enough to say that simply to modify the hypothesis of the rectilinear propagation of light would suffice; and that it is quite easy to arrange things *a priori* when we are sure that actual experience cannot disprove them! For in the first place, as we have seen, this hypothesis could not be accepted with regard to a homogeneous distribution of matter in a medium, without weakening the very foundation which is common to the Euclidean and to the non-Euclidean geometry. And if we should suppose, in a concrete in-

stance, an action of matter on the propagation of light in celestial space, this action would be in various ways subject to new and indirect observations.

For that matter, Riemann's geometry might be verified in some other way. Indeed, according to this geometry, a straight line would be a closed and finite line, though of enormous length, so that a remote star could be visible at the same time to two observers at the antipodes of the earth.

Finally let us repeat once more that, even if the geometrical question of the parallel lines had not happened to be definitely *solved* by optical observations in the field of stellar astronomy, the latter would have been able in any case to confirm the doubt of the non-Euclidean geometers, and rendering it more determinate, to lead us thence to seek its indirect solution in the realm of planetary mechanics.

In affirming the validity of the Euclidean geometry to a degree of approximation that is indistinguishable from exactitude, we affirm that *to-day*, apart from the *accidental* errors that are inherent in the *application* of the hypotheses symbolically expressed by geometry, there is, in the hypotheses themselves, no error which becomes manifest in the examination of their direct or indirect consequences.

The value of the doubt raised by Lobatschewsky consists in the modest confession that we do not know but that an error of this sort may be discovered in the remote future. It is simply this *possibility* that is disputed by those who do not admit the judgment of modern criticism as to the *strict* validity of our geometry.

§ 11. CAN NON-EUCLIDEAN GEOMETRY BECOME AN OBJECT OF INTUITION?

What still remains against this possibility? To the attempt to test the matter by experience there is opposed only the old argument which regards as *impossible* whatever is *inconceivable*, or better, whatever *cannot be made an object of intuition.*

It is true that, notwithstanding the construction of an adequate system of concepts, we experience the *psychological impossibility of representing actual phenomena to ourselves in any scheme that differs from the ordinary intuition of space.* But this *feeling of necessity* that accompanies our mental vision of space, can tell us nothing of its structure, because physical reality is not bound to give satisfaction to us who form ideas of it.

To clear up the question, Gauss adopted a suggestive argument,

which was then taken up by Helmholtz and Clifford, and which is commonly connected with the name of Helmholtz. Let us imagine that there are *superficial animalcules,* flattened out upon a surface, upon which they are free to crawl. Let us endow these imaginary beings with an intuition as to space, which serves to coordinate their sensations, and to direct their movements, in the realm of two dimensions (the surface) which is *their space.*

Two such little creatures, one of which was moving on a plane, and the other upon a slightly curved surface, might both be equally guided by the same geometrical intuition, because they might picture their space as a plane. The facts which in the second case would allow the animal to gain a knowledge of the curvature of his surface, without leaving it (since by hypothesis he is ignorant of the third dimension) are completely analogous to those which to human beings would indicate the falsity of the theory of the parallel lines, taken in a rigid sense. But the facts designated would not be verifiable by these animals, in case the latter were very small in comparison with the surface they inhabited or with its curvature.

And yet, if in such a society of little animals there were philosophers, who knows whether some one reasoning from the intuition he has formed of his own surroundings as a flat surface, would not argue for the absolute and necessary flatness of the surface itself?

§ 12. CONCERNING OTHER POSSIBLE GEOMETRIES.

In his memorial article "Gauss zum Gedächtniss," Sartorius says: "Gauss regarded geometry as a logical structure, only in case the theory of parallel lines were conceded as an axiom. But he had become convinced that that theorem could not be demonstrated, however well one might know by experience that it was approximately true."

We do not know whether the first part of this account really corresponds to the maturer views of Gauss, and we are somewhat inclined to doubt this, when we recall the observations made by him with the theodolite as to the verification of the postulate of the parallel.

However, the opinion attributed by Sartorius to Gauss in the first part of the quotation would not be acceptable. For in *none* of the postulates of geometry can the character of a logical axiom be recognized. And each of the definitions of the fundamental entities of geometry (defective as we saw it to be in its purely

logical aspect) nevertheless contains within itself an assumption about reality.

We have already observed that the fundamental property of the straight line, expressly involves the hypothesis of a certain symmetry of phenomena, that is, a totality of agreements, without which it would not be possible to group so many different classes of facts by means of a *single* geometrical representation.

As to this hypothesis we can only say that it is, for the present, confirmed by all our experiences. It does not seem easy to give a quantitative judgment of the degree of strictness belonging to it, although surely it is safe to assert that its relative accuracy is enormously large.

But even if we accept as postulates the hypotheses that form the basis of the *unity* of geometry, either metrical or optical, we ought not to believe, for this reason, in the possibility of *one single* system of general geometry, a sort of pan-geometry, for which only the question of the parallel lines would remain open.

On the contrary the most recent mathematical researches teach us that endlessly diverse systems of geometry are still possible, not only logically, but even physically. Let us cite for example Clifford and Klein's space forms, which undertake to represent possible physical constitutions of space that are radically different for an observer who is limited to the narrowness of our experience, and for one whose limits are very decidedly widened.

§ 13. NON-ARCHIMEDEAN GEOMETRY AND THE ARBITRARY NATURE OF THE POSTULATES.

In the preceding examples the *postulates* that correspond to the various geometries, express *different physical hypotheses*. The difference, though not verified by actual experiences, still is virtually verifiable in regard to future possible experiences. But we can construct, and have actually constructed of late years, other and logically different geometries, which however do not correspond to hypothetical physical differences. The most striking example is seen in the non-Archimedean geometry, constructed by G. Veronese (1891) and more recently developed by D. Hilbert and his school, in connection with other important mathematical problems.

This geometry starts with the negation of the so-called postulate of Archimedes, which is independent of the other premises of ordinary geometry. Instead this geometry assumes "the existence

of segments such that the multiple of one of them by any whole number however great would always be smaller than the other."

Such an hypothesis, while it has a precise mathematical sense as denoting the relations of certain concepts, has on the other hand *no physical sense,* because it is not in any way accessible to experience: a segment (an actual infinitesimal) less than any assignable quantity in our system of measure, would be found to be transcendentally defined in relation to this system and to our senses (for example the sense of touch), so that the hypothesis of such a segment does not directly express any datum of the sensations.

But even more, this hypothesis has no physical signification, even indirectly. In fact Veronese has shown that the theorems of the non-Archimedean geometry may be interpreted, with an *infinite degree of approximation,* as identical with those of ordinary geometry, so that we conclude that *"the Archimedean geometry and the non-Archimedean geometry, by means of diverse systems of concepts, express the same system of hypotheses as to the relations of position of bodies."*

And so *"postulates expressed in a different form, are capable of expressing the same physical reality."*

The non-Archimedean geometry gives an interesting illustration of this *relatively arbitrary character of the postulates* with regard to the real world. This arbitrary character has been brought to light by F. Klein in an opinion delivered to the University of Kasan, while Poincaré takes it, as it seems to us, in too broad a sense, regarding every postulate as a *convention.* A conventional element does indeed belong to postulates in the sense that "there are systems of postulates that express by means of diverse representations the same physical hypotheses." But beside these systems of equivalent postulates, there are also, as we saw, non-equivalent systems, to which correspond physical possibilities that can be distinguished from one another by means of experience. An arbitrary choice, in harmony with the requirements of simplicity in our conceptions of space, is permissible only when we are dealing with systems of equivalent postulates, or with non-equivalent systems which have not yet been sifted out by means of a complete experience. But in the former case, the choice is purely conventional and free, while in the second case the choice implies an hypothesis concerning facts, and thus anticipates the result of possible experience. It may be that such experience will be inconsistent with this hypothesis.

B. THE PSYCHOLOGICAL ACQUISITION OF GEOMETRICAL CONCEPTS.

§ 14. STATEMENT OF THE PROBLEM.

We have seen that a survey of experience leads us to view various geometries as physically possible. And mathematical criticism even makes clear the possibility of representing the same system of hypotheses concerning real space, by means of various systems of concepts and postulates. But notwithstanding the arbitrary character which thus remains in geometrical construction, it is true that the intuition which exists in every adult mind, makes a choice, in forming the idea of a psychologcially defined space. Thus arises the problem of explaining this intuition,—a problem of much interest to the psychologist. But in what direction are we to seek the *explanation*?

The answer vacillates between two directly opposite views which dispute the field, *nativism* and *empiricism*. From Kant's thesis that "space relations are relations which the mind imposes upon possible sensations" springs nativism, which connects the intuition of these relations with the anatomical, physiological and psychological structure of man. From the thesis that "space relations form part of the data of the senses" (sight, touch etc.) empirical philosophy infers that the intuition, or the mental image of space relations, must be the simple repetition of previous sensations, and so at last is reduced to a sum of perceived cognitions and facts.

Now the premises of nativism and of empiricism both appear to be true, up to a certain point, but the consequences resulting from them seem onesided and incomplete. Nativism speaks as if we could conceive of a soul all formed before the use of its senses, and independent of the external world. Empiricism on the other hand reduces mental action to a passive receptivity.

These two tendencies may be harmonized by an effort to *explain space intuition as a psychological development from sensations, in which we take account of the structure of the subject.*

§ 15. THE BIOLOGICAL PROBLEM OF SPATIAL ORIENTATION.

But the problem grows complicated, for to the psychological investigation we now have added an apparently difficult biological investigation.

To the empirical thesis that the relations of position form part

of the data of the senses, the nativists oppose the view that "a consciousness of the relations of position may be regarded as implied by the very use of the senses (the adaptation of the sentient organ to the object)." Hence arises the problem of the *orientation in space* or of the *coordination of movements with sensations,* to which many think that the psychological question may be at once reduced.

When the terms of the problem are thus altered, the theory of evolution extends the investigation from men to animals, and from the individual animal to the species. And empiricism is joined with the epigenetic view of Lamarck in Spencer's doctrine[1] which regards the knowledge of space as an acquired knowledge derived from the habitual use of the senses, afterwards passed on by heredity, and thus actually fixed in the completed species.

Nativism on the contrary finds its biological expression in the preformistic and neo-preformistic theories. These systematically seek the cause of phylogenetic variation in the mechanical, physical and biological conditions within the living being, leaving to the Darwinian principle of natural selection the task of securing adaptation to the environment.

Now for our part we do not intend to examine the problem of space from this point of view.[2] Suffice it to point out that the substitution of Spencer's doctrine for the primitive empiricism already marks the recognition of the fact that "in the space orientation of man and of the individual animal there is something innate."

What interests us especially in the foregoing is the conclusion

[1] Cf. Spencer's *Principles of Psychology,* vol. II, chap. XIV.

[2] Apart from general biological views, I have considerable doubts as to the fact that we find a progress in the innate coordination of movements with sensations as we ascend the zoological scale. While a baby needs a year to learn to walk, and an adult man who has been born blind and who gains his sight needs three months time to learn to coordinate his sight and his movements, we see a wonderful quickness of adaptation in young chickens (see Spalding's experiments on chickens quoted by Romanes in his book *The Mental Evolution of Animals.*)

Moreover reflection shows that we ought to distinguish the evolution of the organ of coordination from that of its function. The latter tends to pass from the *conscious* to the *unconscious,* what was at first *voluntary* becoming more and more *reflex.* (Cf. Lewes, *Physiology of Common Life.*) The former tends towards the acquisition of a constantly more complex and conscious function, corresponding to the multiplication of the nerve fibers and of their connections. And so, while the variety of possible coordinations increases in phylogenesis, the element that is empirically acquired in the space orientation of the individual animal seems to have an ever increasing importance. And on the contrary the primitive modes of orientation, being quasi determinate, appear rather to belong to the organic structure in the lower forms of life.

that "the development of the coordination of movements with sensations, that is of space orientation in animals as well as in man, is the product of experiences that have taken place under certain anatomical and physiological conditions."[1] We need not investigate whether and how far such conditions should be connected with the mechanical laws of the organism.

But let us positively reject the view of certain neo-Kantian philosophers that these structural, anatomical, physiological conditions of the human organism are reflected in certain *a priori* characters of space intuition, so that they are able to supply geometry with its postulates before the entities of geometry themselves are built up out of sensations. So especially W. Wundt, in his *Grundzüge der physiologischen Psychologie*[2] (Band II, p. 27) attempts to derive the planary structure of intuitive space from the fact that the arrangement of the bones is such as to favor rectilinear movement. On the contrary it is evident that the jointed members are as it were composite pendulums, whose rectilinear movements cannot take place without complicated adjustments. E. G. Heymans[3] undertaking more definitely the psychological explanation of the postulates of geometry, regards the straight line as a "gleichförmige Innervationsreihe"(?), and attempts to deduce *a priori* from this fact the fundamental properties of the straight line. He does this by means of an analytical symbolism, whose application seems to us absolutely arbitrary.

But to such attempts we must offer the serious objection that the fact of orientation, however dependent on anatomical structure, is observed by consciousness only by means of certain sensations (muscular, tactile, visual etc). And the crude data of these sensations are not such as to do away with the arbitrariness which we have found present in the choice of the postulates. Nor do such crude data give to the postulates that character of exactitude which we perceive in the space relations intuitively present to our thought.

We are here dealing with a psychological development, with an ideal elaboration and simplification of the data of the senses, whose subjective conditions must be sought in the very functions

[1] In his study *Ueber die Raumwahrnehmungen des Tastsinnes* (Berlin, Reuter-Richard, 1898) V. Henri finds it especially necessary to assume as congenital in man an anatomical-physiological disposition through which a peripheral excitation calls forth reflex acts, leading the tactile organ to contact with the place stimulated.

[2] Leipsic, Engelmann, 1880.

[3] *Vierteljahrsschrift für wissenschaftliche Philosophie*, 1888, Bd. XII, p. 279.

of the mind, not in the anatomy of the muscular system, the bones etc.

§ 16. PROGRAM OF THE FOLLOWING STUDIES.

Our own analysis will result in representing this development as a *process of association and abstraction, and in reducing the conditions already mentioned to the laws of the logical operations.* In these we shall find the explanation of certain formal characters of the postulates, and of the feeling of self-evidence or of necessity that accompanies them.[1]

More precisely, after having ascertained the *origin of the concepts of geometry,* whose relations are expressed by the postulates, we shall see that these postulates consist of two parts. One of these parts consists simply of the statement of a fact that is somehow evident, but which is viewed as a *condition* needful for uniting, in a single concept, a certain series of diverse members which is capable of endless extension. The other part is reduced to a principle or to an axiom, of a logical kind, which is connected with the logical function of thought already analyzed (Chap. III).

By a figure one means a real object that corresponds to several associated sensations. And so the existence or the possibility of an hypothetical object corresponding to a given group of sensations constitutes the geometrical fact expressed by the postulates. On the other hand the *form* in which these sensations are presented to the intuition of the geometer, and the self-evidence connected with this form—these are expressed in the *logical propositions that are superadded to the fact itself.*

§ 17. SOURCES OF OUR CRITICISM.

But we cannot go deeper into the sense of the explanation thus sketched, unless we carry further the critical study of the genesis of geometrical concepts. The facts which claim our attention belong on one side to physiological psychology, on the other to geometry. It does indeed belong to physiology to show how the relations of extension are perceived by sight, or touch, or by muscular sensations. But in order to interpret these results properly we need to know in what way the relations perceived are connected with the fundamental concepts of geometry. The physiological point of view by itself cannot get beyond the problems of

[1] Cf. F. Enriques, "Sulla spiegazione psicologica dei postulati della geometria," *Rivista filosofica,* Pavia, 1901.

the discrimination of simultaneous sensations, or of the localization of sensations, or of the determination of a position in space. But these problems, as G. Cesca[1] justly observes, hardly touch the psychological problem as to how we get our notions of space.

Helmholtz clearly saw the necessity that is here presented, of directing and interpreting the physiological aspect of our investigation in the light of a critique of the postulates of geometry. But it seems that this lesson was later forgotten, and for that very reason the experimental work carried on in various places, has not brought all the profit that it seemed to promise.

Meanwhile the researches into the principles of the science of geometry, which in Helmholtz's day were just beginning to get help from instruments invented for the work of higher mathematics, have now, thanks to these instruments, gained an enormous development. And the results obtained during the past half-century, are such that no physiologist who is working on the psychological question of space, can dispense with a knowledge of them.

§ 18. GENERAL OBSERVATIONS AS TO THE SPATIAL CONTENT OF SENSATIONS.

A space-content is found or can be found in every sensation. With dogs it seems that smell plays a large part in orientation. And in the case of men who are born blind, space ideas are habitually connected with hearing, according to Hitschmann's testimony.[2] But, as a rule, in man it is the muscular sensations and those of touch and sight, that in connection with the movements help to form by association our ideas of space.

The objects of the latter are *points, lines* and *surfaces.* Lines may be *generated* by moving a point, and surfaces by moving a line. But nevertheless, lines and surfaces, in so far as we conceive them as real facts, correspond to groups of sensations, which are different from those given by the motion of the point or line in question. Hence results a twofold aspect of lines and surfaces, to which we wish to call the reader's attention; that is, their *genetic aspect* and their *completed aspect.*

We should regard the complete representation that we form of a line or of a surface, as derived by association from the two groups of sensations that correspond to the two ways of considering it.

[1] *Le teorie naturistiche e genetiche della localizzazione spaziale.* Verona-Padova, 1883.

[2] *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* III, p. 388.

But, not only the line or the surface, but also the *point,* should be regarded not as the *object of a sensation,* but as *corresponding to a group of associated sensations.*

To affirm the existence of a point has first of all a significance relating to the position of the observer, and implies the assertion that given muscular sensations, corresponding subjectively to certain movements, will be accompanied or followed by given sensations of touch or sight. Only in this sense, namely in the sense of a correlation between an experience and a receptive attitude toward the experience, can we speak of the "sensation of a point."

This view leads toward the solution of the problem of space localization according to the so-called *associative theory* of Bain, Taine and Delboeuf.

Let us add that the *discrimination of simultaneous sensations* of touch or of sight becomes possible by means of the so-called *local signs* of Lotze, that is to say, by means of taking account of the mutual differences between points on the skin and on the retina, especially with regard to their relations with neighboring points. We thus have a sufficiently satisfactory solution of the problem, which aims to explain physiologically "how we succeed in *determining a position in space.*"

§ 19. SPACES IN PHYSIOLOGY AND SPACE IN GEOMETRY.

Since the finding of a point, as a crude fact, depends upon the position of the observer, and with the change of his position the relations between the points appear to the subject as different, it follows that the totality of the points, thus understood, constitutes in relation to the observer himself a *physiological space* that is different from *geometrical space.* This fundamental distinction appears in the *Beiträge zur Analyse der Empfindungen* by E. Mach,[1] and in Hering's studies quoted by him (*op. cit.*).

Other authors also have called attention to this fact, that physiological space, visual or tactile or muscular, has not the characteristics of homogeneity and isotropism that we regard as belonging to geometrical space. See H. Poincaré in his work already quoted. This point, however, appears to us so evident, that it seems useless to insist upon such differences.

But from the establishment of such a difference arises the problem of explaining how we pass from the crude data of the

[1] Jena, G. Fischer, 1886; English edition, Chicago, Open Court Publishing Co., 1897.

sensations of space to geometrical ideas. Here we are obliged to add to the sense associations a *process of abstraction.*

To say that *"the geometrical concept of space is the abstract of the various physiological spaces that are possible in relation to a moving observer,"* may not perhaps be considered quite a sufficient answer to the question that has already been stated. For it does not show us, on the one hand, how, thanks to association and abstraction, the transition is made from physiological space to a quasi-geometrical conception of space *in relation to* the observer. And on the other hand it does not show how all the elements of special asymmetry in the various physiological spaces, can be eliminated in the course of the process of comparing them.

Leaving the first task to a more accurate analysis of the data of the senses, let us observe concerning the second, that in consequence of the action of gravity a systematic asymmetry arises in relation to the movements of the observer. This was observed by Mach.

From this we think it allowable to argue that the passage of our intellect from the physiological to the geometrical idea of space is especially due to the comparison of those possible *visual spaces* into which have already been introduced the elements due to association with other senses, for this mechanical asymmetry does not exist in connection with optical phenomena.

A greater difficulty, inseparable from a more intellectual process of abstraction, must then be met in the origin of the concept of space in those who are born blind. And a similar difficulty must then be seen in the origin of the *mechanical idea* of space, as we shall later find a better opportunity to observe.

We will next pass to the discussion of the special conceptual elements of the idea of space, that are furnished by the various senses. The manner of treatment must be such as to render our views accessible to those who have had no special mathematical preparation in this field, and such as to show more plainly the need of using mathematical considerations in an argument of this sort.

§ 20. THE SPACE DATA OF SIGHT AND PROJECTIVE GEOMETRY.

Concerning the numerous results as to the physiology of sight (with which the observations and experiments of Lotze, Weber, Volkmann, Fechner, Helmholtz, Panum, Donders, Hering, Wundt, James, Mach etc. deal) it must suffice for us to draw up a funda-

mental scheme of the process, without entering into a minute discussion of its details.

The elementary facts of vision are as follows:

1. The formation of the *image* on the retina, equivalent to a central plane projection of the object.

2. The *movements of the eye,* normally taking place around the so-called Donders's center, and approximately obeying the *law of Listing,* and the law of *constant orientation* which depends upon Listing's law.

3. The normal *fusion* of the corresponding *images* in *binocular vision.* In this connection we should notice that the fusion normally takes place, not only when the images fall on (identical) points occupying an analogous position in the two retinæ, but always, within certain limits, when the images correspond to the same external point, so soon as, by the aid of various concomitant sensory data, we have obtained the suitable *adaptation* of the organ. The single point is seen almost exactly in the direction of the ray which bisects the angle of the two visual rays.

Having stated these premises, we are now ready to discuss the fundamental and constantly disputed question *"whether sight gives an immediate perception of the distance between two points, that is, of the size of the object."*

The simple fact of binocular vision after adaptation, appears as a *bicentral projection,* that is as a duplex and simultaneous projection of the object from two centers upon two planes.

If we ask a mathematician what can be inferred from such a representation, he need only turn to those studies of photogrammetry, which are contained in the recent works of Hauck and Finsterwalder.

Two projections of the object suffice, as is plain, for its reconstruction, when we have *given* the *system of projection,* that is to say, when the respective centers and the respective images are given. And then, if the images of two points are given, the distance between these two points can also be determined on the basis of certain *metrical elements* which belong to the *system* in question. These elements are the distance of the centers of vision from the respective retinae for each given adaptation.

When such elements are lacking, four images of an object suffice for the reconstruction of an object geometrically similar to the real one. Five images (amongst which the necessary connections hold) determine in addition the size of the object.

How ought we to interpret these results in connection with our problem? The visual data contained in the many images which can be formed through the movements of the eyes include elements from which we can mathematically deduce the mutual distances of points in the object. But this *has no special meaning* with regard to the separate images. Moreover the judgment of distances cannot even be regarded as immediately given by binocular vision, because the metrical elements of the corresponding system of projections are partly anatomical data, partly the variable data of accommodation, and cannot be regarded as *known* to the observer in advance of visual experience, and also in advance of those comparisons and associations with other senses which this experience implies.

Thus the extension of the visual faculty to the perception of the form and size of objects must be acquired by an empirical development.

The necessity of taking account of the data of other sensations associated with sight, in this development, appears also from the theory that considers the sense of sight as becoming differentiated during evolution from the general tactile sensibility. We must indeed admit that there is a certain immediate and purely subjective perception of distance, according to which we perceive with the eye the *apparent distance* between two points, on the basis of the length of its projection upon the retina. But this element of innate judgment, related as it also is to the muscular sensations which go with the movements of the eye, can be rendered accurate only by a larger experience, in which the movements of the observer and other connected sensations take part.

The above inductions are based upon the anatomical and physiological consideration of the organ of sight. They are well corroborated by a study of the psychological side of the question.

Among the many facts established by Helmholtz (*Physiologische Optik*) and by his school, we especially wish to call attention to the *error* which is often met with in the *comparison of distances.* Experience has shown that this comparative judgment (the practical possibility of which is limited to a somewhat narrow field) does not show the characteristics of uniformity and accuracy, excepting when the distances compared are equally remote from the eye. And furthermore this estimate becomes very imperfect when the distances lie in different directions. We may also add the error in the estimation of the parallelism of two straight lines,

which is more perceptible than that which belongs to the recognition of their convergence etc.

Helmholtz has explained these very facts, maintaining that the comparative judgment of distances is not an immediate datum of vision, but an acquired judgment due to the habit of associating the data of sight with those of the muscular sensations and the sense of touch.

From all these preceding statements we feel authorized to accept this distinction as well founded. We admit then as an *immediate datum of sight,* the complex of those geometrical properties of the object which are translated into *properties of its projection.* independently of the particular position of the object, and of its distance from the eyes. Lengths and distances do not appear among these immediate data, and must be regarded as empirical acquisitions, obtained in a *mediate* way, by association with the tactile and muscular sensations, together with those visual sensations themselves which arise from a change in the position of the observer.

But such conclusions can only be justly interpreted by one who has gained certain fundamental notions of modern geometry! And precisely to the ignorance of this subject are due those strange conclusions over which some philosophers are still toiling.

Since sight alone gives no comparative judgment of the distances between points, and hence of the size of the objects, it might be believed that every geometrical notion is foreign to the immediate data of sight.

This error can only be corrected by a study of projective geometry, as developed during the past century, from Poncelet to Möbius and Steiner, until it reaches its definite and autonomous form in the theory of Staudt.[1]

Only in this way may we gain the notion of a science which studies the *qualitative relations* that are inherent in the elementary concepts of the *straight line* and of the *plane surface.* These relations are completely independent of the *quantitative* (or *metrical*) relations which are presupposed by the idea of distance, although they are ordinarily expressed by means of the latter.

It is precisely the *straight line and the plane* which through their optical properties are *made manifest by the sense of sight*: the straight line because its images upon the two retinae are straight, and the plane through its relations with the straight line. For

[1] Cf. the historical appendix to the *Lezioni di Geometria proiettiva* by Enriques. Bologna, Zanichelli, 1898; 2d ed., 1904.

these relations determine a *projective correspondence*[1] between the two retinal images of the plane.

These considerations plainly refer to binocular vision. Meanwhile in monocular vision the straight line and the plane still have a character of their own, when they pass through the center of the eye. The straight line is then a *visual ray*, and its image is reduced to a single point. The plane appears as a straight line. These particular representations of line and plane are combined with our general ideas of these objects and determine, as we shall see, certain fundamental properties of our intuition of the line and plane themselves.

It is worthy of notice that, in binocular vision, a curved surface is distinguished from a plane, since the sensation of *relief* is connected with such a surface.

Relief then is a character of the visual representation of a surface belonging to the actual appearance of this representation itself. The retinal correspondence now differs from the projective correspondence in such a way that the lines of the surface which have as their images straight lines on one retina, have as their images curves on the other retina.

This method of distinction, being connected with the immediate data of binocular vision, has nothing to do with the muscular sensations that accompany the movements of the eyes when they pass from looking at a curved surface and then look at a plane surface. In the first case we have a criterion of the curvature which depends, in a certain sense, upon the mere consideration of the surface in itself, that is of its *internal* properties. In the second case the curvature is estimated by the (external) relations of the surface with that which is outside of or beyond it.

We have then, together with the possible derived representations of a surface, also a direct representation of certain surfaces, in their relief. And this is especially the case when we observe a surface which limits our visual horizon. So far, although, as we must hold, not further, we must go in admitting that there is a *direct representation of objects of three dimensions.*

Except in the respect now indicated, three-dimensional objects correspond in our thought to a succession of images of surfaces, or to the abstract conception based upon such a series of images, but

[1] This is what we call the correspondence of the points of two plane surfaces, whose straight lines correspond.

in vision they are never presented as an actual datum of the sensations.

The arguments to the contrary advanced by Stumpf[1] seem to us to include a vicious circle. The great psychologist attempts to derive the necessity of a representation of three dimensions from the fact that a surface has *two faces,* centers of curvature *outside* itself etc. But these considerations already introduce implicitly as given the third dimension which the psychologist is trying genetically to construct! But as a fact the direct representation of a surface, viewed as the limit of a visual horizon, contains *only one face,* so that the concept of two faces comes from superimposing *successive* images etc.

To say that the immediate data of vision give us the notions of straight lines and planes, amounts to affirming that *vision furnishes us with the constructive elements of projective geometry.*

And so the immediate space of the sense of sight, as we get it by abstraction from possible visual spaces, if metrical associations are excluded, would be a *projective space.* And since every straight line can be optically conceived as prolonged, that projective space may be represented as *unlimited.* As a fact, this boundlessness is conceived somewhat tardily. Indeed a decided effort of abstraction is needed to reach this notion, on a basis of the verified enlargement of the visual horizon when we travel toward it.

In any case there remains one peculiarity of the optical representation of space, that in it we never find immediately compared two distant points lying in opposite directions. From this it follows that, in the projective space of sight, the two rays drawn through a point, and parallel to the same straight line, do not seem necessarily to coincide. Their coincidence at least could not be immediately recognized, and so long as it can not be, the projective space of sight will not have acquired in our minds all the extension that we attribute to it. In mathematical language, projective space, in its more immediate optical representations, will appear as a body within a *limited improper surface,* of the second degree. We shall later see how the associations of touch force us to enlarge this projective space still further, until we reach the Euclidean representation, in which the limiting improper surface is reduced to a plane.

[1] *Ueber den psychologischen Ursprung der Raumvorstellung,* Leipsic, 1873 (*passim* and especially p. 177 and later.)

§ 21. THE SPATIAL DATA OF THE TACTILE AND MUSCULAR SENSATIONS, AND METRICAL GEOMETRY.

The physiology of the tactile-muscular sensations presents involved and complex problems; the more so because the questions that belong to the various special senses, depend for their solution upon an understanding of the tactile sense. For from the point of view of evolution, the special senses appear as differentiations of the general sense of touch.

The distinction commonly received between the senses of pressure and temperature, does not perhaps entirely exhaust the various qualities of the sense of touch. And greater doubts might arise as to the sensory office of the various nervous expansions which are found in the epithelial tissue, such as the so-called *tactile corpuscles* and Vater's corpuscles.

We meet with still graver difficulties in the explanation of the muscular sense; or the sense of muscular motions, and in determining their relations to touch. It will suffice to indicate the controversy as to the existence of a *sui generis* sensation, accompanying the centrifugal nervous current which moves the muscle. Under the name of *sense of innervation* the supposed sense here in question takes a large part in the views of Wundt, and is also assumed by Helmholtz and Mach, but on the other hand is denied by James as an unnecessary hypothesis.

Fortunately it seems that these questions do not need to be answered before undertaking the task that we propose. It will suffice us to recall certain fundamental facts:

1. The tactile sensations, and in particular those of pressure, are referred to the place on the skin that has been touched (Lotze's *local signs*). But in determining that place we commit an error that varies considerably from one to another position, so that two points which in a certain position on the skin mark out less than a certain interval between them (*the threshold of sensation*), are no longer perceived as distinct.

2. A constant length is perceived as unequal upon different parts of the skin, that is it appears greater where the fineness of the sensations is greater. (H. Weber's experiments with the compass).

3. The size of a moving object upon the skin is perceived more perfectly than is the size of an object that is stationary upon one part of the skin, and this faculty noticeably increases with practice.

4. To the sense of pressure we find opposed the *sense of effort*

and the *sense of muscular movement.* With the latter is connected the as yet ill-defined office of the *semicircular canals* of the ear in case of the sensations of equilibrium and of orientation (Goltz and Mach.)

Certain observations as to the spatial content of the tactile-muscular sense result from these facts. We may notice the experiments of Fechner and Weber, relating to the threshold of the sensation of touch and to the unequal perception of equal lengths.

The threshold of sensation belongs not only to touch but to sight. It has a very instructive meaning inasmuch as it shows that *physiological space* (of touch or of sight) *is not continuous.*

Hence arises the problem of explaining how the process of association and abstraction, by which we gain the geometrical idea of space from the possible physiological ideas, leads to a *continuous space.*

The second point to be taken up is the following: The comparative judgment of distances or lengths, and more generally of the size of objects, requires reference to an organ of touch that is chosen as the seat of constant comparison. This organ of touch should be movable while remaining unchanged, and adaptable to objects, in order that different objects may thus be metrically compared. It becomes a true organ of *special exploring touch,* differentiated with reference to the metrical distinctions.

The hand is normally such an organ, but a foot or some other part of the body is often actually substituted for it in fulfilling certain offices.

We must further notice that the special exploring touch just mentioned quite quickly enables man to perceive the invariable element in the motion of solid bodies, and hence to learn that these can be utilized as more precise instruments of comparison and measurement.

A point passing over the skin, or a knife-blade laid quietly upon it, give us two tactile images of a line. The first gives us the genesis of the line, the second the completed line. If the blade passes over the skin scraping it lightly, we have the idea of the genesis of a surface. The idea of the completed surface, distinct from the former and only connected with it by association, corresponds to a strip of metal, of a certain length laid upon the same skin.

The form of the surface, and in particular its flatness or its curvature, may be given to us by the completed idea of the surface

referred to the parts of the skin which have been fitted to the surface. Thus, for example, different sensations of muscular contraction evidently correspond with the extended or the incurved position of the hand.

But the tactile representation of the three dimensions is only obtained genetically. It is given by the succession of the impressions of touch caused by a moving surface, in contact with a part of the skin that moves with it. The special exploring organs of touch have in all this a more important office, if not a necessary one.

Amongst all lines and surfaces we especially distinguish *circles* and *spheres,* the recognition of which is a datum of exploring touch. The formation of these ideas is connected with the comparison of distances.

As to the *straight line* and the *plane,* the distinction between them seems to us to be, at least largely, a mediate acquisition. It is true that certain bones are approximately rectilinear, but this particular characteristic could not attract our attention until we had noticed other important properties of the straight line, such as the fact that it forms the *axes of rotation* of solid bodies, or that it is the *line of least length.* A muscular definition of the straight line is also possible, based upon its mechanical properties. But when we compare everything, it would seem that the *primordial* constructive elements which most conspicuously enter into the formation of the *tactile-muscular space, are not the notions of the straight line and the plane, but of distance,* and therefore of *circles* and *spheres.*

These last conclusions may be expressed by saying that *tactile-muscular space,* and especially the space mapped out by exploring touch, *is metrical space,* whose properties result from the fundamental properties of *distance.*

That this space is *unlimited,* and that the straight line in this space is unlimited,—these properties result from the possibility of so moving the exploring organ of touch or any object whatever as to produce a succession of equal lengths that can be carried out indefinitely.

For the rest, the *infinity* of space itself does not necessarily follow from the above statement. These consecutive lengths placed in line one after the other (an operation which in the abstract should be conceived as endless) might well return upon themselves, after a sufficiently long interval had been traversed. Only ex-

perience tells us that this does not happen in any region accessible to us. Metrical space, so far as the conception of it is furnished by the elementary tactile-muscular experiences, could then just as well be a Euclidean space, or a space as described by Lobatschewsky, or a space such as Riemann describes. And the choice of Euclid's conception, which is attainable even by those who are born blind, must be indirectly suggested to them by other more complex experiences. On the other hand we shall see later how for the normal man this is a result of the union of experiences of touch and sight.

§ 22. PARALLEL BETWEEN THE HISTORICAL DEVELOPMENT AND THE PSYCHO-GENETIC DEVELOPMENT OF THE POSTULATES OF GEOMETRY.

Just here it will be well to return briefly to the developments relating to the principles of geometry which (as we pointed out in § 9) were carried out in various directions under the influence of Riemann. The comparison of these researches with the results just established will enable us to see that *geometers have succeeded in dividing geometry with respect to constructive elements which correspond to the various groups of sensations.* At the same time we shall thus reach a general classification of the geometrical postulates for which a psychological explanation is needed.

The researches into the principles of geometry[1] may be distinguished according to three fundamental tendencies: the *elementary tendency,* the *metrical tendency* and the *projective tendency.*

The first tendency attempts a *logical* systematization of Euclid's school of thought, and leads to the foundation of the non-Euclidean geometry, of which we have already spoken.—In this last stage of the investigation the *physical* interest of the question of space is more important than the logical standards of arrangement. However, this elementary tendency is characterized by the lack of any analysis which might tend to *separate the concepts of geometry.* All the fundamental concepts (the straight line, plane, congruence etc.) are considered side by side, and the only effort is to *simplify* the propositions or postulates which express their primary relations, and so to throw light on their interdependence.

The separation and the more searching criticism of the fundamental concepts of geometry are more characteristic of the stage of research which was begun by B. Riemann, the deepest philosopher of geometry. Here, through the direct influence exerted by

[1] Cf. Enriques's article already quoted.

Herbart on his disciple, the *psychological criterion* also enters the field as a guiding interest. For the rest, the relations of the concepts of geometry are henceforward to be studied by the aid of the higher mathematics.

The line of study which was indicated by Riemann, and with which Helmholtz was also associated, constitutes the *metrical tendency*, according to which all geometry is founded upon the notion of *distance* or of the *length of a line*, within a continuous manifold. These studies are connected, on the one side with the development of *differential geometry*, founded by Gauss, and on the other with the *theory of groups of transformations*, as it has been later brought to light by F. Klein, S. Lie, and H. Poincaré.

Meanwhile, the foundation of projective geometry, and the already mentioned independence of metrical notions given to it by Staudt, led Klein to begin on a new tendency (the projective). In following this tendency the investigation of the principles assumes as fundamental the (*graphical and optical*) notions of the *straight line* and the *plane*.

From this time onward the connections between the graphical and the metrical classes of concepts occupied the minds of many investigators, while others devoted themselves in various ways to investigating the intrinsic relations of metrical or of projective geometry, connecting these investigations with profound and interesting mathematical problems.

But the projective geometry and metrical geometry have a qualitative substratum in common, in the sum total of the relations, belonging to the more general concepts of *line* and of *surface*. These relations characterize a *continuous manifold of several dimensions*. These concepts, without the intervention of any such ideas as the straight line, the plane, congruence etc., already give rise to a branch of geometry, called the *theory of extension*, or of the continuum, or *Analysis situs*, which however has not been very fully developed by any one method.

Consequently a critical study is in order which shall deal with these leading principles of all geometry, a study which may on the one hand be connected with certain views barely outlined by Riemann, and on the other with certain studies by Dedekind, Weierstrass, Cantor etc. The problems which thus originate were plainly stated and in part geometrically treated by the present author, in certain special researches (cf. § 24).

Without going further in this hasty review, let us bear in mind

that the critique of the principles of geometry has led to the differentiation of geometry itself into two branches, the *metrical* and the *projective*. These have their common root in the general *theory* of the *continuum* (or *Analysis situs*).

We can now see that these two branches are connected with two groups of sensations, the tactile-muscular, and the visual. And we may even add that the general tactile-muscular sensibility, which apart from the specialization of touch is also the foundation of the impressionability of the retina, already gives us the geometrical ideas belonging to the theory of the continuum, that is, the notions of lines, surfaces etc. On the other hand, as we have said, the estimation of distances and hence the genesis of an exact idea of congruence or of geometrical equality is due to a special exploring organ of touch.

These conclusions may be schematically summed up in the following statement:

The three groups of ideas that are connected with the concepts that serve as a basis for the theory of the continuum (Analysis situs), of metrical, and of projective geometry, may be connected, as to their psychological origin, with three groups of sensations: with the general tactile-muscular sensations, with those of special touch, and of sight, respectively.

This result guides us to a psychological explanation of the postulates of geometry, and in its turn is confirmed by such explanation.

§ 23. THE POSTULATES OF THE CONTINUUM: THE LINE.

Let us now turn to the postulates of the theory of the continuum, with special reference to the concepts of the line and of the surface.

The concept of the line, we have said, is derived by means of association and abstraction from the possible ideas both of the genesis of a line, and of the completed line.

Viewed with reference to its genesis the line is a series of points, arranged according to the temporal order given in the mind (cf. chap. V). The series arranged in the opposite direction is combined with the former series, both uniting in the idea of the completed line which, from a certain aspect, represents their abstract. Thus we obtain the first fundamental postulate, the *invertibility of linear order*, which, according to Herbart and Bain, marks the difference between linear and temporal order.

We must still give an account of the *continuity* of the line, which is expressed in two postulates:

1. Between two points on a line there is always an intermediate point.

2. Dedekind's postulate. If a line is divided into two parts (classes of points) in such a way that:

a. Every point on the line belongs to the one or to the other part;

b. Every point in the first part precedes (in one of the two orders of the line) every point of the other part,—

then there must exist a *point of division,* which follows every point of the first part of the line, and precedes every point of the second part.

Let us turn our attention to the first postulate.

It is at once clear that this postulate cannot express a property belonging to any series of points empirically given. Also we have already shown that, in physiological space, this property of the line is contradicted by experience.

When the distance between two points A, B on a line is less than twice the threshold of sensation, it becomes impossible to place between them a point that shall be perceptibly distinct from the two given points. However, in many auxiliary ways we ordinarily show that this impossibility can be done away with either by passing to a more sensitive portion of the organs of touch or of sight, or by refining our powers of perception in any way.

Since various sensory images are combined in the concept of a line, we should explain the preceding fact by recollecting that a point C, between A and B, may fail to be distinct in a sensory image, by reason of a *defect of perception.* The acceptance of this assumption is also favored by the fact that the threshold of sensation is not absolutely fixed, that moving the object upon the skin may lower the threshold etc.

This conceived extension of experience, then, leads us to think that it may be possible to place a point between A and B, on a given line, even if this is not immediately evident to the senses. However this extension has in fact a limit that is soon reached. If then the concept of the line must be formed in our minds by combining all the sensory images of one single line, physically given, we shall never be led to postulate that the interpolation of a point between two given ones could be carried on indefinitely.

But let us suppose all possible lines to be combined in one

single class of concepts, and let us try to determine the characteristics of the abstract concept which would thus result.

The combination of two lines, l, l' implies ideally a one-one correspondence between the points of these lines. And experiment suggests to us that such a correspondence may be obtained by making two distinct points A, B on the one line correspond with any two other points A′, B′ on the other. This follows from the superposing of elastic threads stretched to different degrees of tension, from the establishment of a projective correspondence by means of two straight lines etc. Now, since two lines l, l' can be combined in this way, we must needs think that between the two points A and B on l, as also between A′ and B′ taken at suitable distances on l', we could always interpolate another point. This indefinite possibility of interpolation is then a necessary property of the concept of a line, in so far as that concept represents the ideal product of the combination and abstraction of all the empirically attainable ideas of the genesis of lines.

But the continuity of the line is not yet altogether characterized. To what is the second part of it due;—the part described by Dedekind's postulate?

That the process needed for this task must be more laborious, is shown by the fact that the Greeks never grasped the complete idea of the continuity of the line, even though they were obliged to go beyond the first stage that we have already described, when they were studying the problem of the intersections of straight lines and circles.

In dealing with this problem we must in fact assume that the segment connecting a point within a circle with a point outside the circle, intersects the circle. It is plain that in making this statement we are adding something to postulate 1, because the sum total of the points on a straight line which have *rational* abscissae with respect to a certain unit of measure, would constitute a system of points such as to satisfy postulate 1. But no one of these points would in general coincide with a point of the circumference according to the above hypothesis.

The postulate about the intersections of straight lines and circles may be regarded as a particular case of Dedekind's postulate, a case in which there is a division of the straight line into two parts, one of which is composed of points *"within"* the circle, the other of points *"outside"* it.

But the Greek geometers did not generalize this case by exam-

ining all the analogous divisions of a straight line that can be made. Or at best they made only some attempts in this direction, with which are connected certain well-known sophisms that have come down from them to us.

The analysis of continuity is an acquisition of modern science which has been most fully and accurately expressed in Dedekind's postulate.

That " a point divides a line into two parts" is an immediate consequence of the linear order. Dedekind's analysis adds to this the knowledge of the conditions under which "the division of a line into parts is accomplished by means of a point." These conditions correspond to the representation of the point as the *terminus* or *dividing entity* of the line, regarded from the point of view of the completed line.

Postulate 2 as to continuity expresses, then, a condition by means of which we can associate two different ways of representing "the point," (1) as an element which creates the line, taken in the genetic sense, and (2) as the terminus or dividing entity of the line itself when taken as a completed line.[1]

The genetic explanation of the concept of continuity is a problem of such importance that it is worth while to clarify it from another point of view, assuming, in place of the postulate of Dedekind, the logically equivalent postulate of Weierstrass: "Any infinite sequence of points belonging to a given segment always possesses *a single limit.*"

Let $A_1\ A_2 \ldots A_n \ldots$ be the infinite sequence of points in question,—defined by some recurrent construction,—these points belonging to the segment a and being arranged (for example) in an order proceeding from left to right. If we choose some one of the concrete images or representations of the segment a, say the image a_n* then from a certain point onwards, all the remaining points of the sequence would become indiscernible. For example, from the point A_n onwards, the points $A_{n+1}\ A_{n+2}$, etc. would be seen as constituting together a single point. But the value of the index n which is in question may vary with the choice of the image

[1] This explanation of the psychological genesis of the continuum, set forth by the present writer in a paper of the year 1901 and already quoted, is closely related to the discussion of H. Poincaré in his recent book *Science et Hypothèse,* already often quoted.

* The author means by the "concrete images" of a the appearances that a would have if it were viewed by the eye at a certain degree of nearness or under some magnifying glass of a certain power, so that such and such parts of it could be distinguished.—J. Royce.

or representation a_n of a;* so the number n may consequently be made as large as we choose by choosing a sufficiently magnified image of a.

Hereupon the hypothesis that, in the segment a (as we represent it ideally), there exists a point A which is the limit of the series $A_1\ A_2\ A_3 \ldots . A_n \ldots .$, means simply this: There exists a series of segments A_n A, each of which stands at the ends of some set of points $A_1\ A_2 \ldots .$ and each of which in the case of *some one* of the representations of a, appears as a point whose extremes are indiscernible. To this series of segments we make the ideal point A correspond.

This method of association is the simplest constructive process which can serve to form an abstract conception of a line, but it is not the only possible method. If now we drop the condition that the two representations of the "elementary generating point" and of the "dividing point" of a line shall be so associated as to correspond to a single abstract entity, we can construct, with Veronese, a continuum of a higher and non-Archimedean order, in which there exist actual infinitesimal segments, and for which the equivalent postulates of Dedekind and Weierstrass no longer hold good, but only a less restrictive postulate of continuity, that is known by the name of Cantor.

Leaving aside, however, such abstract constructions of the non-Archimedean geometry, let us sum up those results that belong to the ordinary intuitive notion of a line:

The postulates about the line, considered in its internal aspect, express the possibility of combining in a single abstract concept, according to the logical laws of thought:

1. *The ideal genesis of a line as empirically given, and its inverse, in the order of time;*

2. *the various ideas of the genesis of the same line or of different lines; united with the ideas of completed lines, in such wise that we unite in a single abstract notion the two series of images of the point as a generative element and as the dividing element of the line itself.*

Other properties of lines, connected with the notion of a surface, arise from the comparison of the ideas of the completed lines, but these are concerned with the *external* relations of lines, while

* That is, the author means, if you choose a higher magnifying power you would discern more of the points as they cluster closer together towards the right.—J. Royce.

the concept of the line, regarded internally, will remain henceforth fixed by means of the constructive procedure that has been described.

§ 24. POSTULATES AS TO THE CONTINUUM OF TWO AND OF THREE DIMENSIONS.

The general concept of the *surface* as a *manifold of two dimensions,* is genetically derived, according to Riemann[1] from the movement of a line.

Interpreted in the broadest way, this method of generating the surface leads us to take as given upon a surface, *two pencils of lines,* the *generatrices* and the *directrices* or trajectories described by the points of a moving line.

If then we suppose a *metrical system* to be established on these lines, that is, if we suppose the notion of *linear length* to be given, we are able to represent the points on the surface by a *system of coordinates.* That is to say, we establish a continuous one-one correspondence between the points of the surface and the pairs of numbers of a numerical manifold of two dimensions.

If we do not wish at the outset to postulate the metrical system for the lines in question, the introduction of the coordinates no longer seems possible merely on the basis of the data which appear in Riemann's account of the generation of a surface. For this reason the latter is to be regarded as an insufficient definition of a manifold of two dimensions, and S. Lie explicitly adds the hypothesis of the one-one correspondence with a numerical manifold of two dimensions.

For the purpose of clearing up this point, which is so essential to the pure theory of the continuum, we have undertaken a research[2] whose results we will here set down: We can introduce the coordinates upon a surface, whenever we have given, upon it, *three pencils* of lines intersecting two by two at a single point. For this purpose it suffices, substantially, to admit *two* different *modes* of *generation* of the surface itself by the motion of one of its movable lines.

This theorem not only answers the mathematical question how to *define* the continuum of two dimensions,[3] but also the psychological question of explaining the postulates which relate to them according to ordinary intuition. Indeed such *postulates,* contained

[1] Habilitationsschrift, Göttingen, 1854. (*Göttinger Abhandlungen,* XIII, 1868.)

[2] Enriques, *Circolo Matematico di Palermo,* t. XII, 1898.

[3] Cf. the author's article already quoted.

in the hypothesis of numerical representations, *express the conditions which make it possible to combine in a single abstract concept of the surface the various modes of generation which are connected with it.*

To the postulates that enable us to define the surface, regarded internally as a manifold of two dimensions of points, we may add those postulates which characterize the *external relations of the lines on a surface.*

With this kind of relations we class the property of the *division* of a surface *into parts* by means of *dividing lines,* and hence the existence and the parity or the disparity of the intersections of this terminal line with another line, which connects two points of the different parts etc.

The conception of such properties is accomplished in a constantly more determinate way by extending the concept of a"line on a surface," so that gradually, certain completed lines taken as dividing lines are combined in the mind with the generating line of a given surface.

If for example we start with the conception of the plane as generated by its straight lines in the various possible ways, there arises the mental image of a semi-plane bounded by a straight line, and hence we have the postulate of Pasch, which expresses the following *properties of division*:

If three points A, B, C in a plane are such that the segment BC does not intersect the straight line a and the segment AB does not intersect it, the segment AC will intersect a; that is if B, C are on the same side of a and A, B on the opposite side, then A, C will be on the opposite side.

Now the question of stating the postulates that are capable of defining *in a general way* the concept of a "line on a surface," meets with the difficulty of setting a limit to a constructive process of the intuition which starting with a certain family of lines, gradually is extended to more general families. For this reason it is doubtful whether and in what sense the mathematical analysis of the problem can lead to a result that may in any way be regarded as complete.[1]

Nevertheless these observations have already pointed out the psychological method of acquiring the postulates under discussion.

[1] Let us note the various results of Cantor, Peano, Jordan, and some observations on *analytical lines* etc., to which the reader may refer in the author's article on "Principii."

What happens is the combination of the idea of a completed line viewed as the dividing line of a surface, with various images that refer to the possible ways of generating this surface.

The transition from the continuum of two dimensions to that of three does not give rise to any really new observations. The postulates which describe the generating surfaces serve also to define this continuum.

We will limit ourselves to calling attention to the fact that the *postulate of the three dimensions* expresses a limitation of the genetic process by which we pass from points to lines, and from lines to surfaces etc.

For the rest we have already noted that the continuum of three dimensions, viewed as a finished whole, is not itself the object of a direct experience (cf. especially § 20).

As to the *external relations of surfaces*, we have here also a progressive construction which passes from particular families of surfaces to more general families.

§ 25. POSTULATES OF PROJECTIVE GEOMETRY.

We have shown how the postulates that serve as a basis for the theory of the continuum form conditions that make it possible to combine, in the abstract concepts of the line and the surface, the various conceptions of genesis and of finished product which belong to the forms in question. Our critique has at the same time brought to light a certain indeterminateness in these general concepts, which depends upon their relativity, and from which we see that a progressive construction is necessary, which first posits certain particular lines and surfaces, and proceeds from these to gradually extend the concepts already defined.

This necessity shows us in a certain sense the office of that evolution by which the particular sensations of sight and of special touch are differentiated from the general tactile-muscular sense-perceptions, for the purpose of gaining notions of space.

The sense of sight emphasizes the straight line above all other lines, and in forming the concept of the straight line it unites two quite distinct ideas,—distinct, that is, from each other and from other ideas of lines.

The straight line appears in fact, as a line which if not passing through the visual center, has rectilinear projections, and it also appears as a line (or visual ray) which if passing through that center, and if seen by one eye, is projected into a point.

Now the combination of these two ideas presupposes the *existence of a line which is seen from every one of its points as a single point*, and hence involves the *postulate regarding the determination of the straight line, i. e.,* Any two points belong to one straight line which is equally determined by any other two of its points.

In fact let A, B, C be three points on a straight line, which is viewed from its point A, and let T be the image on the retina (or the trace on the plane of the retina) of the straight line itself. In this case the point T is just as much the image of B as of C. Hence the straight line AB can be optically defined as the locus of those points whose images fall on T (the image of B), and the straight line AC may be similarly defined as the locus of those points whose images fall upon T (the image of C).

By the principle of identity, the same definition defines the same fact. That is:

$$AB = AC.$$

But by means of the representation of the right line as not passing through the visual center, the straight line AC appears as identical with CA, and since we can here repeat the previous considerations with regard to another point D:

$$CA = CD.$$

Thus we definitely ascertain the identity of the straight lines that are determined by the pairs of points AB and CD.

While this postulate expresses a fact upon which depends the possibility of combining under a single abstract concept the idea of the visual appearance of the same straight line, it is no sooner viewed as a report of fact than it is also translated into a conceptual equivalence. This is the explanation of that feeling of necessity which goes with the postulate in our reasoning.

Similar considerations can also be applied to the *postulate of the plane.* "A line lies wholly in a plane when it connects any two points in that plane." If in fact the plane is derived from the *projection of a straight line from a point external to it,* the postulate just mentioned tells us that "if we choose any two points of a plane not aligned with the center of vision, and project the line which these two points determine from any one of the points of the plane, we determine the same plane."

Hence we infer that this postulate expresses the conditions by means of which we are able to combine, in a single abstract concept, the various representations of a plane surface, and in particular

those which correspond to the view of that surface from any one of its own points.

Thus *the postulates belonging to projective geometry are seen to be conditions which render possible the combination of certain visual representations, from which the abstract concepts of the straight line and the plane originate.*

§ 26. THE POSTULATES OF METRICAL GEOMETRY.

The postulates of metrical geometry deal with the *movements* and the *congruence* or *geometrical equality* of figures, abstractly conceived as *solid bodies* that are penetrable or capable of being superposed.

The first properties to which we must call the attention of the critic, are those relating to the *possibility* and to the *degree of freedom of motion* of solid bodies. When one point of a figure remains fixed, another point may describe a surface. When two points remain fixed, another point chosen in general may describe a line. When three points chosen in general remain fixed, no motion is possible.

The fact is rather significant that in some incomplete analyses of these notions (as for example in that of Hoüel), the properties just mentioned appear only among the postulates, while the properties by virtue of which geometrical congruence falls under the logical concept of "equality" are rather regarded as *axioms.* And indeed the postulates we have mentioned appear in the first place as the statement of *mechanical experience.* Their kind of certainty is not accompanied by such a feeling of necessity as that which belongs to the fundamental properties of congruence, but it has only the kind of *evidence* which arises from *qualitative experience.*

Helmholtz seems to have been the first to point out that geometrical congruence cannot be regarded as an equality, without implicitly admitting other essential facts concerning motion. M. S. Lie, H. Poincaré and D. Hilbert, each in his own way, have made the matter clearer, by subjecting these facts to a new form of criticism.

The fundamental view by which these researches have been inspired seems to have been first pointed out in the *Erlanger Program* by F. Klein (1871).

It is as follows:

A movement in space, or in a region of space, posits a one-one correspondence of the systems of points in question, (a trans-

formation). In order that the relation of two figures that may be transformed by a motion (congruence) can be regarded as an equality it is necessary that:

The successive combination of two movements has as its result or product a transformation which is still a movement (whence if A = B and B = C, A = C).

The *inverse* transformation of a movement must also be a movement (whence if A = B and B = A, it follows that A = A).

This may be briefly expressed by saying that "movements form a group of transformations." Plainly such a statement has the character of a statement of fact, which includes certain properties of the *relative invariability* of solid bodies in motion and of the organs of touch, independently of the way in which the latter passes from one position to another.

Now, granting that the postulates about congruence have, when regarded objectively, just as much empirical value as other postulates of geometry, how can we explain the decided feeling of psychological necessity by which they are accompanied?

For the sake of simplicity let us consider two congruent figures, made up of two pairs of *equidistant* physical points, AB, CD; and let us suppose for example that the distance between the points of one pair is *three finger lengths.*

If we place the three fingers between A, B and between C, D, we shall have two successive sensations which differ according to the position in which the organ of touch has been placed, but which have something in common that belongs to the invariability of the organ itself while it is passing from one position to another. The ideas of the two pairs AB, CD are not identical, but they can be combined in a single idea, by means of *something that they have in common,* i. e., by means of the concept of their *distance.* Now when we state the equidistance

$$AB = CD$$

we mean to express at the same time an identity and a non-identity; that is, the possibility of bringing the two ideas under a single idea in respect to *certain relations,* and the distinction of the latter with respect to *other relations.*

What then is implied by the genesis of the concept of equidistance, which serves as the foundation of geometrical congruence? An abstraction and a combination which unites, in certain respects, two ideas. And the *postulates about* congruence referring to the

unified concept therefore take the form of the logical axioms about equality.

§ 27. METRICAL-PROJECTIVE COMBINATION: THE POSTULATE OF THE PARALLELS.

Thus far we have taken account of the conditions of combination which underlie the genesis of the concepts of geometry, in relation to a single group of sensations, those of touch or of sight.

These have a common substratum in those concepts that are derived from our general sensibility, and as soon as the two images, the tactile and the visual, of a point are combined, they give rise to two kinds of geometry, the tactile and the visual geometry, which refer to the same continuum of three dimensions, that is to say, to the same space.

The mathematical developments to which we have referred, especially those belonging to the last half of the nineteenth century, show us the degree of development that is possible to these two kinds of geometry. Hence we may declare that from touch (both general and special) we can construct a *metrical* geometry, and from sight a *projective geometry,* both referring to the same manifold of points, that is, to space.

Nevertheless these two geometries are connected so as to relate to a single realm of space relations. And each of the senses, (especially sight) gives us through association the mediate perception of all these relations.

Such an association in its turn implies new postulates. For in fact it would be possible to construct, in a manifold of three dimensions, two abstract and conventional geometries, using as a basis for one a linear system ∞^3 of surfaces called "planes," and as a basis for the other a group ∞^6 of transformations called "movements," endowed with certain properties. And these two kinds of geometry, which would be formally identical with the projective and with the ordinary metrical geometry respectively, would stand *a priori* in no mutual relations.

Ordinary geometry on the other hand unites in a single concept of the straight line both its visual and its tactile images, thus recognizing the same *physical symmetry* in optical and in mechanical phenomena.

This union is so close that it takes profound study to separate the two classes of associated concepts. At first sight we do not see the difference between the fundamental optical property of the

straight line, namely that it is determined by two points, and its mechanical property, namely that it is the line of the least distance, or the axis of rotation of a solid body. The essential reason of this is to be found in the fact that the union of the properties under discussion is implied by the judgment "a line congruent to a straight line is a straight line." If in the phrase just written we place the word "equal" instead of "congruent," quite a critical effort is needed to see that the judgment thus expressed is not reduced to a mere tautology.

However, if we suppose that "light does not travel along the shortest line, that is along a straight line defined in the mechanical sense," the path of a ray of light made to rotate around two of its points would give a line congruent to the first, but which could not be regarded, like the former, as a "sraight line" in the optical sense of the word.

The congruence of straight lines then expresses the fundamental physical fact that belongs to the propagation of light in homogeneous media. This fact makes it possible to combine the two kinds of geometry (the tactile and the visual) in a single metrico-projective geometry, in such wise that in the most complete geometrical construction, congruence will still wear the logical aspect of an equivalence.

It may be noticed in fact that by an act of abstraction, similar to that which we have analyzed in the field of movements, we reach in the same way the notion of a *projective* or visual *equivalence,* a relation dealt with by geometers under the name of *projectivity.*[1]

Now metrical equivalence (congruence) appears to sight as a particular case of projectivity. Thus it comes to denote the identity of the space relations of two figures considered in their *internal* aspect. Hence the metrico-projective postulate about the congruence of straight lines is hidden under the form of an axiom about logical equivalence.

Some have even thought that from this point they could arrive at a definition of congruence as the identity of the internal properties of two figures. But even apart from its vague character, this definition is insufficient. In fact, without comparison with something external we cannot distinguish congruence from geometrical similarity.

Since metrical space, which is derived from the ideas provided

[1] A transformation which keeps straight lines straight and planes as planes, and hence keeps all the *graphic properties invariant.*

by touch, and projective space which is constructed from those furnished by sight, are fused in the abstract concept of a single metrico-projective space, the idea of the latter is determined first of all, as that of an *infinite space*. The unlimited nature of the straight line from the point of view of optics and of mechanics, and its optical aspect as an "open line," exclude indeed Riemann's conception.

However we still have to explain how Euclid's postulate can be reached while Lobatschewsky's hyperbolic conception is excluded. In this connection it is natural to compare the two ideas, tactile and visual, which we form of *parallel straight lines*. These appear in their optical aspect as straight lines in a plane which do not intersect, and more precisely, as the limits of pairs of straight lines intersecting at points more and more distant. From the point of view of touch they appear rather as *equidistant lines*.

The combination requires us *to conceive two parallel lines as equidistant straight lines* (in a plane). And the hypothesis as to the existence of two such straight lines implies, as is well known, Euclid's postulate about the parallels. In other words, the combination leads us to perceive as single a straight line whose rays are optically parallel to another given straight line in both its opposed directions.

Thus then, the *postulate of the parallels arises from the combination of touch and sight which brings us to the metrico-projective concept of space.*

This concept, however we may discuss its exact value in reference to reality, remains always Euclidean in its subjective formation. But critical study, which has gradually distinguished the elements entering into the construction first succeeded in analyzing those most complex and recent associations which bind together the data of the different senses. From this point research has gone further and further back, to the very origin of the process of formation.

Can the foregoing explanations of the genesis of Euclid's postulate be confirmed by the study of the geometry of persons born blind? And on the other hand would these explanations find any support in the history of non-Euclidean geometry?

It would seem that the answer to the first question must be, if the postulate of Euclid arises from an association of touch and sight, that those who are born blind would have no intuitive notion of it.

But this supposition is *a priori* inadmissible, since it seems clear that, lacking sight, other tactile-muscular experiences lead by means of their complex to an Euclidean representation of space. And this is actually the case. We have been able to verify the fact ourselves.

Meanwhile we may suppose that those who are born blind would hardly experience the usual repugnance for the non-Euclidean hypothesis. It would be quite interesting to clear up this point. In the institution of Bologna an experiment has been tried with a person who had been born blind, and who had had some training in geometry, but to me that experiment really seemed insufficient. And it would seem rash to accept too readily as a proof of our thesis the testimony of those who, in assenting to the possibility of the doubts that we have expressed in the non-Euclidean manner, might evidently be receiving a suggestion from our very questions themselves. What we have actually got from such questionings is a belief in the great difficulty of reaching any certain conclusion in this way.

History on the other hand furnishes a confirmation of the genetic explanation of Euclid's postulate. Our friend G. Vailati has told me indeed that Saccheri attributes the slender progress made before his time in the non-Euclidean criticism, to the acquiescence of many geometers in the definition of parallel lines as "equidistant straight lines," a statement which is for Saccheri an "*error of complex definition.*" And it is from the detection of this logical error that this ingenious scholar received the impulse for his original critique.

But what then is a complex definition, if it is not an implicit way of postulating the existence of an entity, the concept of which is derived from several combined conceptual constructions?

With equal right we might say more generally, that the so-called logical necessity of the postulates of geometry, always rests, in a certain sense, upon a complex definition, while, on the other hand, the ideas which are combined may not be, in their separateness, definitely conceived at all.

§ 28. CONCLUSION.

From a synthetic point of view the foregoing considerations as to the way in which the concepts of geometry are acquired, bring to light the variety of elementary and unconsciously repeated experiences which are called up again in the visualizing or intuition

of space. But still more they show us the long process of association and abstraction by which these concepts themselves were formed.

The *self-evidence* of geometry must mean, on the one hand, the ease with which former unconscious experiences can be recalled, by repeating them, so to speak, in a simplified form by means of mental imagery; on the other hand it must imply the possibility of comparing in the *form* of the postulates the operations of combination and abstraction, as they have been applied to the elements of which space is built up.

The facts presupposed by the postulates are thus seen as the conditions required for a psychological development, which takes place according to the laws of logic and the principles of economy, and which thus succeeds in framing a more unified conception of geometrical reality.

V. MECHANICS.

THE OBJECTIVE SIGNIFICANCE AND THE PSYCHOLOGICAL DEVELOPMENT OF THE PRINCIPLES OF MECHANICS.

§ 1. MECHANICS AS AN EXTENSION OF GEOMETRY.

IF we disregard those doubts which criticism casts upon the rigorous validity of the postulates of geometry, as a means of predicting possible future experiences, geometry then appears as a purely deductive science, the model science of rationalistic philosophers!

But this character of logical perfection belongs to it only as a theoretic doctrine, that is, as a statistical rule (Chap. IV, § 7). If on the contrary we consider the results of geometry as to their objective value, in their relation to concrete cases, that is if we take geometrical relations as a part of physical relations (Chap. IV, § 6), the deductive developments become incapable, from a certain point onwards, of furnishing us any more precise prevision.

The separation by abstraction of geometry from physics is useful only so long as these cases are considered as given in their totality, under *undifferentiated* physical conditions. But where, on the contrary, it is possible to classify the *differences* of these cases, so as to estimate the systematic influence of these differences upon previsions, we then are able to *extend geometry by completing its concepts by associating with them new sense data,* in such a way as to render the scientific application of geometry more determinate.

In this sense *mechanics forms a first extension of geometry,* since it succeeds in making a definite choice among the various relations of position of bodies that are regarded as geometrically possible, always taking account of time, force, mass etc.

Now these new elements are not only combined with space data, but also can be *subordinated* to geometrical representation

in the process of forming mechanical concepts, precisely because this process *comes after* geometry *as a psychological acquisition.*

We have already observed (Chap. IV, § 6) that such an ordered realm of concepts, as mirrored in the order of dogmatic exposition of science, has not the value of a *necessary subordination.* There is nothing to hinder us from altering this order, that is, from constructing a science of mechanics independently of any of the hypotheses of geometry, while the latter could then be expressed by an equivalent mechanical hypothesis. In this way mechanics may lead to a more extended verification, or even to a correction of the very principles of geometry.

Let us take for example Euclid's fifth axiom about parallel lines. Ordinary statics is founded upon this axiom, but a more thorough examination shows us what a non-Euclidean form of statics would be. Two equal forces perpendicular to a rigid rod AB at its extremities would give a resultant perpendicular to AB at its middle point 0, precisely as in the Euclidean case; but, herein differing from the latter case, the resultant would not be equal to the sum of the two components. (Foncenex, Lagrange, D'Alembert, Genocchi, De Tilly). Here then we have a static expression of the geometrical axiom about parallels, a new assumption which we perceive to be contained in it. And the verification of this assumption within the limits of rigorous experiment, constitutes a new proof of Euclidean geometry. If on the contrary this verification had shown us an appreciable difference from our prevision, it would have led to a correction of the very principles of that geometry.

But the developments of mechanics have not given rise to corrections of this sort, so that we may say that *a more extended verification of geometry has been furnished by mechanics.* In certain phenomena it has seemed rather difficult to verify the principles of mechanics. But even in these it has not seemed possible to provide a systematic explanation of the discrepancy by altering any of our previously accepted geometrical principles. For the rest this conclusion agrees with the kind of exactitude possessed by geometry, the kind deduced from more direct verification (cf. Chap. IV, § 10).

§ 2. PROGRAMME.

Since we wish to consider mechanics as an extension of geometry, it would seem natural to treat the matter in the following order:

1. First of all an analysis of *time,* since this concept arises as parallel to space, and is found already formed in advance of the scientific development of knowledge. The addition of time to space leads at once to the class of ideas which constitute *kinematics* or geometry extended by motion.

But, since a thorough discussion of *motion* brings the ideas of kinematics into relation with those of dynamics, we have postponed this discussion, assigning it to the third part of our programme.

2. A critique of the concepts and principles of statics.

3. A critique of the concepts and principles of dynamics.

The separation of statics and dynamics is in accordance with historical tradition which no one has ventured to change till very recently, and is also prescribed by our method of treatment of forces.

We will also add that for the purpose of bringing closer together the two methods of treatment, and of adopting in dynamics the accepted simplification, we have at first limited ourselves to the *mechanics of points,* although in statics it might have seemed an advantage to consider *systems* also at the outset. So questions relating to systems will be discussed in the latter part of the chapter, in connection with statics as well as with dynamics.

§ 3. TIME: SUCCESSION AND DURATION.

We notice that in case of two sensations or of two groups of sensations (phenomena) one is *before* and the other *after,* or else they are *contemporaneous.* The *intuition* that we express by the words "before and after" gives a standard for the arrangement of phenomena in serial order (temporal succession).

In speaking of two phenomena A, B, and of two others C and D, we are in the habit of comparing also the *interval of time* that has elapsed between A and B, and also between C and D, by saying that they are *equal* or that the one is *greater* than the other. This judgment leads us to a quantitative valuation, that is to a *measure* of time or of *duration.*

It is clear that the measure of duration, whenever it is established, involves the criterion of temporal succession. But on the contrary *the notion of before and after does not furnish any criterion for comparing two intervals of time* which have not a common beginning (or end).

§ 4. PSYCHOLOGICAL TIME AND PHYSICAL TIME.

Judgments of time, either of succession or of duration, may be referred to various *series of phenomena,* in the same way that judgments as to length may be established in relation to different series of bodies which are compared as to lines. Every series of phenomena gives us in this sense a scale of time, in which we have given at least before and after, and in which we may have given a comparative standard of duration.

The abstract concept of time arises through combination and abstraction from the concept of all possible scales of time.

Abstract time, which we conceive as *physical time,* is in reality then the *assumption* of a temporal scale in which all *possible phenomena* find their place, thus differing from *physiological* or *psychological time* which is the scale of all *perceived phenomena.*

The assumption of physical time implies in particular:

1. The possibility of combining the temporal ideas of different observers.

2. The possibility of combining temporal ideas in relation to different places.

The agreement of the combined ideas and the consequent verification of the previsions arising from them, will bring to light the *objective character of time.*

Suppose we have three phenomena A, B, C, and three observers, one of them observes A and B, while the second observes B and C, and the third observes A and C. If the first observer finds that

B follows A,

the second that

C follows B,

we can foresee and in actual fact verify that for the third observer

C follows A.

This is the signification of the agreement of the temporal ideas of different men, and we can clearly see how in this is implied the possibility of combining several temporal scales in relation to these observers, in a single scale in relation to a hypothetical observer.

The combination of temporal ideas in relation to different places, meets with a difficulty in the following fact. Certain sensations which we associate by thinking of them as inherent in some localized phenomenon, and which appear as contemporaneous to an

observer on the spot where the phenomenon occurs, appear on the contrary as successive to an observer who is in a different place. Thus it is for example with light and sound when a gun is discharged.

In order to make the needed combination, there intervenes then an hypothesis:—that is that the partial phases of a phenomenon, which are successive in space, must be also successive in time (time of propagation). And this hypothesis is confirmed by experience in case of phenomena that are reflected back to their initial point.

And so, in order that we may establish a combination between temporal ideas which relate to different places, we need

Either to find a phenomenon which is propagated instantaneously,

Or to correct the empirical judgment of the contemporaneous occurrence of phenomena in different places, by measuring their time of propagation.

Disregarding the process of measurement, of which we shall soon speak, the quasi instantaneous propagation of light or of electricity serves practically to establish the desired combination.

But the concept of contemporaneousness thus accepted is not rigidly accurate, and even contains an appreciable error. In fact we know that Roemer on the basis of certain astronomical considerations, and Foucault by a direct experiment on this earth, have measured the velocity of light (about 300,000 km to 1″), a result which was afterwards found to express also the velocity of the propagation of electric waves.

Disregarding then the criterion of measure, temporal succession is thus rendered independent of space only in the degree of approximation indicated by the velocity of light. In the latter degree of approximation, which is very high in the common acceptation of accuracy, we can easily verify upon the earth the concrete previsions to which the hypothesis of such independence leads.

§ 5. THE PROPERTIES OF TEMPORAL SUCCESSION.

The properties of that series which we conceive as physical time are imposed upon us by the conditions under which the various possible scales of time are combined.

If we should take one single scale made up of a periodic series of phenomena, this would give us a representation of time as a *closed series.* But the variety of our sensations in their complexity forces us to consider time as an *open series,* which then is extended

indefinitely in both directions by the assumption of possible phenomena, following or preceding those of the given series. The periodic series then may be combined with non-periodic series, by virtue of contemporaneousness, so that to each phenomenon corresponds a series of repetitions.

The *continuity of the temporal series* is an hypothesis relating to the concept of physical time, to which we are led by our desire to combine in one abstract temporal scale all possible temporal scales, and notably those that have to do with our genetic images of the various *linear series* of tactile or visual points (cf. Chap. IV).

Psychological time is not continuous, because all phenomena perceived one after another are *moments* in it, and appear as a *numbered series.*

§ 6. DURATION.

The notion of an order according to which phenomena follow one another does not exhaust the content of our knowledge of time. We usually add to it a metrical determination of duration which is in the highest degree relative to the series of phenomena that is taken as a temporal scale.

A criterion for the measure of time is given in general by any series of acoustic or visual phenomena which follow one after another at distinct intervals, but yet near enough together to fill the whole mental field of attention. The quantitative judgment of duration obtained by observing such series is furnished by *counting* the pauses.

In a continuous visual series, for example in the series of impressions corresponding to the tracing of a line described by a moving point, we can establish a measure of duration by associating the intervals of time with the distances described by the moving point, which we measure according to their length.

These criteria, referring as they do to one or another series of phenomena taken as a temporal scale, give us a purely *relative measure* of time. The measurements furnished by different acoustic or visual series cannot be compared one with another. The established measure that refers conventionally to a particular series, serves merely to establish a more precise mode of prevision of the "before" and "after," within the series chosen as a standard of measure.

By means of a conventional measure of time we are able to make successive moments correspond to the values of a variable t,

which expresses the interval of time that has elapsed since a certain moment fixed upon as a point of departure.

If instead of t we take an *increasing arbitrary function*

$$\tau = f(t),$$

we have another conventional measure of time, which may have some concrete significance in relation to another suitable temporal scale.

As opposed to this concept of the measure of time, a concept which is purely relative to the various series of successive phenomena, we have the view entertained by Newton under the influence of medieval philosophy. This view assumes an *absolute time* independent of phenomena, in which there is given a *true or mathematical standard for the comparison of durations.*

This view is justly disputed by Mach[1] who gives historical, psychological and linguistic reasons.

It is enough for us that in this view we meet the same transcendental way of interpreting the process of abstraction which we have often criticised already.

If then the notion of absolute time is seen to be senseless, must we conclude that the measure of time is purely relative? Most critical geometers seem to come to this conclusion, as for example P. Volkmann[2] and apparently Mach, although less explicitly, adheres to the same view, in which the measure of time is compared to that of temperature.

To get rid of the arbitrary nature of the substitution

$$\tau = f(t),$$

which figures in the measure of time, one conventionally chooses a scale of reference, by measuring the intervals of time as proportional to the spaces traversed by a moving body upon which no forces are acting. Thus one takes the *principle of inertia* no longer as expressing a relation between force, motion and time, but as a *definition of time.* But this definition, note well, cannot in any way be reduced to a pure *convention* because it implies a supposed fact.

[1] *Die Mechanik in ihrer Entwickelung,* 4th ed., Leipsic, Brockhaus, 1901, (pp. 232-237).

[2] *Einführung in das Studium der theoretischen Physik.* Leipsic, Teubner, 1900. Cf. also Poincaré's article on "La mesure du temps" which is contained in the volume just published entitled *La valeur de la Science.* It might seem strange that some considerations developed in this book for the justification of the nominalistic conclusion, so much resemble some of the arguments used by the present author on the opposite side. But the fundamental cause of the difference lies in the transcendental view of the French philosopher (cf. Chap. IV), to which view the spirit of our critique is opposed.

"If two moving bodies, not acted upon by any force, traverse two spaces, a, b, in the same time, they will also pass through spaces proportional to a and b in the same time."

But the foregoing is in any case too narrow a view of the facts that are found to be implied in the ordinary measure of time. Suffice it to note that this measure existed before the discovery of the principle of inertia!

Common sense believes in a *natural measure of time* to which it confusedly attributes an actual physical value of its own. What significance can this opinion possibly have when we have eliminated from the question every transcendental concept of a time independent of phenomena?

Let us compare the notion of the measure of time to that of temperature and of length.

Time, like temperature, and like a portion of a line or (in particular) a segment of a straight line, may be represented by a numerical variable t which increases in a given way.

Every increasing function $f(t)$, taken instead of t will lead equally in the three cases to the representation of the *ordered succession* of times, of temperatures or of linear lengths. And we can construct a conventional *clock,* a *thermometer,* and a standard of *measure,* corresponding to an arbitrary choice of the function $f(t)$.

For the clock, it will suffice to refer to an instrument that is based upon the suitably varied movement of a swinging pendulum through a quadrant, and which indicates *equal durations* corresponding to those intervals of time during which the function $f(t)$ receives equal increments. For the thermometer it suffices that the dilations of the substance in the tube shall be estimated upon a scale on which we designate as *equal increases of temperature* the increases corresponding to the *equal increments* of $f(t)$. These conventionally equal increases of temperature will be for the rest in various relations with the increases in volume of the substance in the thermometer tube, according to the nature of the substance chosen. As to the standard of measure, we could make a conventional one, according to the condition fixed upon, by using a *graduated elastic thread,* differently stretched in its different parts, provided however that we so arrange things that any pull of the thread will always reach the same degree of tension when by a movement it takes again the same position.

Now the various conventional thermometers equally well repre-

sent the sensations we experience with the increase of temperature. Indeed the sensation that we experience in passing from 0° to 1° and that experienced in passing from 20° to 21°, as measured by their relation to an ordinary mercurial thermometer, are not in any way comparable, wherefore, if another thermometer shows us the second increase as double the first, we have in our sensations no criterion whereby we can give a preference to the one indication rather than to the other.[1]

But we cannot say the same of the various possible conventional measures. In fact just one of all these measures, that made of a *solid inelastic cord,* agrees with the *sensations of the length of a line* that are given us by the organ of touch. We therefore regard that measure as a *natural measurer of lengths,* and we perceive that the possibility of this method of measuring presupposes a *general fact,* that is, the *property of invariability of solid bodies* in relation to our organ of touch, and the invariability of these bodies in respect to one another when they are in motion.

Now what shall we say of the clock? Is there a clock the indications of which correspond to a sensation of duration in relation to certain clearly distinct series of phenomena which we have in mind when we speak of a *natural measure* of time? And is there any general fact that is expressed by the reciprocal agreement between the indications given by the temporal scales we have mentioned?

In fine, beyond the sensations that go with counting the pauses in any series of distinct phenomena, does there exist a sensation of duration relating to *isochronous* series of phenomena, which can be compared with the tactile sensation of length? Or, on the contrary, has duration no unique signification for the senses, as is the case as to quantity in the increase of temperature?

We will now try to answer these questions. We have the *sensation of rhythm* in the case of certain acoustic series that we call *isochronous.* The various series of sounds that we feel as isochronous give us measures of time that can be compared with one another. They lead, that is, although within somewhat narrow limits of sensation, to one same judgment of *equal durations,* and hence to one same *natural measure of time.*

That hearing affords a very precise judgment of isochronism, and that there is an agreement in the sensation of musical rhythm, is an undeniable fact, at least in the case of trained musical ears.

We may however doubt whether what we have here is an

[1] As to the signification of absolute temperature cf. Chap. VI.

original sensation of hearing, or rather the result of an association with certain muscular movements, which in a series of successive repetitions we tend to make regularly.

Every one knows that musicians make use of a process of this sort when they are *beating time.* And the isochronism of the slowly repeated movements of the hand or foot, is perhaps connected with the rhythm of certain organic functions and especially of the pulse.

We might say however that the ear gives us the notion of isochronism, and hence of duration, by means of association with the muscular sense, just as the eye gives us the notion of length by association with the data of touch.

But whatever is our view of this matter, the fact remains that "by means of a series of repeated movements accompanied by sounds, we can establish a measure of time, in regard to which men agree within restricted but sufficiently precise limits, *as if* they perceived duration as an actual object of sensation."

And since we constantly verify predictions with regard to sensations of duration, we can speak of *duration* as of *a real thing.*

Any clock the indications of which correspond to the sensations we have described, should then be regarded as an *instrument for measuring physical time,* in which respect it differs from any other conventional clock whatever. And in such a clock we shall then have the means of *extending the natural measure of time* beyond the narrow limits within which we are confined when making use of an isochronous musical series.

The agreement between the sensations of duration at once secures us a *certain* agreement between naturally graduated clocks, but a *more precise agreement,* wherever it is found will constitute a general fact which we intend to recognize.

§ 7. THE POSTULATE OF THE MEASURE OF TIME.

In extending the comparative judgment of durations beyond the limits of primitive sensation, we follow the method usual in analogous questions.

Let us start with the assumption that the *equality of durations* expresses an objective characteristic of phenomena within more precise limits than those fixed by immediate sensation. Then we shall be led to assume that this equality ought to appear in case of all phenomena taking place under determinate and *constant conditions,* and attributed to quantitatively equal causes.

Although such an assumption has not a rigorous meaning, it immediately leads us to investigate the following questions:

1. Whether phenomena that are produced under certain *similar* and practically constant *conditions,* which we regard as the effects of *perceptibly equal causes,* have durations that will appear to our senses to be equal.

2. Whether such phenomena, when compared beyond the limits of direct sensation, lead to one same judgment of the equality of durations.

3. Whether in case disagreement arises in the attempt to verify assumption 2, we may not make the disagreement smaller by progressively eliminating, as far as in us lies, certain notable inequalities in those phenomena that are regarded as equal.

The assumptions that correspond to questions 1, 2, 3, constitute the *fundamental postulate about the measure of time,* to which we appeal directly or indirectly in the construction and correction of clocks. *The agreement between clocks* thus constructed, within the limits of possibility, constitutes the verification of the postulate itself. The truth of the postulate may be hypothetically regarded as *rigorous,* so long as we meet with no *systematic errors,* but only *accidental errors* which tend to be eliminated by taking their average.

Let us try to estimate the extension of the postulate thus introduced. With this aim let us reflect that:

1. On the earth we already have a first perceptible agreement between the various sand or water clocks, and to speak more generally, between those founded upon constancy of weight. And a more precise agreement may be gained once we have recognized by the means already described the isochronism of the small oscillations of the pendulum, by using this principle as a fundamental assumption for the construction of pendulum clocks.

2. Astronomical observations lead us to regard as equal certain intervals of time during which quasi periodic phenomena of movement take place. For example, we are thus led to assume the constancy of the sidereal day, the month, the year etc.

And the various criteria of measure thus obtained agree precisely enough with the measure of terrestrial clocks, and still more precisely among themselves. Furthermore the agreement obtained can always be made more precise by means of suitable corrections and comparisons, according to our fundamental postulate.

It is truly remarkable that the degree of exactitude of the verifications to which we have referred reaches a limit of practical

accuracy. And it is not really strange that small differences should appear in the measures furnished by clocks, when we think of the many causes of variation that exist among conditions that are regarded as equal. Such conditions are the variations of temperature which affect the fall of water or sand through a small hole, or the length of a pendulum, changes whose effects we cannot well regulate. Other conditions are the actual causes that can modify gravity in a given part of the earth; the conditions of the planetary system, which are slowly changing apart from the periodic variations; and even the variations of the astronomical universe which we regard as constant conditions!

Now that the facts assumed in the measurement of time have been made clear, it seems fitting to say that we are not in any way authorized to regard them as *necessary truths.* If two water clocks once empty themselves in the same length of time, how is the possibility excluded that in some later trial one of them might become empty sooner than the other?

Against the possibility of this hypothesis we in vain bring forward the feeling of *evidence* that accompanies the postulate about the measurement of time. Indeed such evidence depends only upon the fact that *the postulate expresses the condition that is necessary in order that the duration may be combined with other sense data in the concept of a phenomenon.* But the verification of that condition, properly speaking, constitutes the experimentally established fact with which we are dealing, namely, *the objective reality of duration.*

§ 8. TIME AS INDEPENDENT OF PLACE.

Let us grant the postulate analyzed in the previous section. The measure of time is then established *in a given place,* in such wise that the variable "time" is determined at every instant except for an integral linear substitution:

$$\tau = at + b.$$

The constants a, b depend upon two arbitrary conventional choices, namely, upon the choice of the unit of measure, and upon the choice of the initial moment.

In order that we may compare two periods of duration *in different places* A, B we may use either one of two criteria: We may carry a given phenomenon from A to B or *vice versa,* making use of the postulate about the measurement of time; for example we may carry the water clock or the pendulum clock etc., paying due

attention to any corrections for conditions such as the variation of gravity and so on.

2. We may signal to B the beginning and end of a phenomenon taking place in A, by means of light, or electricity etc. In this comparison the time of propagation may be neglected, considering that it may be regarded as constant, as was shown by the postulate in the preceding section, provided that the distance between A and B does not vary.

That the two standards of comparison agree in one same judgment as to equal durations in different places, we may regard as implied by the very postulate already referred to, when taken together with the fundamental geometrical assumption of the symmetrical constitution of phenomena (the homogeneity of space). But this is true only under the condition that A and B shall be *relatively at rest,* and after we have verified the fact that the intensity of the phenomenon has no influence upon the velocity of propagation.

Let us imagine a double and reciprocal signaling from A to B and from B to A, by means of light for example. The two times of propagation ought, under equal conditions, to be regarded as equal in view of the aforesaid symmetry. Hence we may possess the means of directly measuring the time of propagation employed from A to B, in all cases where the signal can be reflected back to A. We only need to halve the interval traversed from the start to the return of the light wave or the electric wave, or whatever means is used.

This principle is the basis of the measure of the velocity of light that has been given us by the experiments of Foucault.

Now it is clear that we may thus succeed in fixing, with as much theoretic precision as is desirable, the judgment of contemporaneousness in different places, always under the condition of relative rest.

Of course the experimental agreement of different judgments which we can thus obtain constitutes a fact. But the postulate of this fact should be regarded as contained in the postulate about the measuring of time.

Therefore by virtue of this fundamental postulate we can:

1. Judge of the equality of duration in different places A, B.

2. We can judge of the contemporaneousness of two phenomena in these two places;

provided that A and B are relatively at rest.

Thus far we seem justified in *transferring the same numerical representation of time that has been established for a place A, to any other place B which is relatively at rest with regard to A.*

It is in this sense that *time is independent of place.* But this is a *relative independence.*

If B varies in relation to A, that is if the distance AB increases or diminishes, the test of signaling (optical, electrical etc.) no longer permits us to determine the initial moments as simultaneous. In case we simply adopt this criterion, as if A and B were fixed points, we shall find in the time estimate a *constant* increment depending upon the place, which can be observed only in case it is possible to make use of the other test, that is the bringing of a clock from A to B.

But there is more to be said. Under the mentioned conditions the test of signaling will not even serve to establish the equality of two time intervals relating to phenomena that take place at A and B respectively. The time will be also affected by a multiplicative constant depending upon place.

To sum up: *When places in relative motion are concerned, and when it is impossible to move a clock from the one place to the other, the time is determined except for a linear substitution*

$$\tau = at + b,$$

in which a and b are local constants depending upon the relative velocities.

This idea of *local time* plays an important part in Lorentz's electro-magnetic theory and in Poincaré's most recent developments to which we shall refer in Chap. VI.

§ 9. THE HISTORICAL DEVELOPMENT AND THE EVIDENCE OF THE PRINCIPLES.

In his classic work *Die Mechanik in ihrer Entwickelung,*[1] E. Mach makes a profound study of the historical development of the concepts and principles of mechanics, taking up and discussing the problems and special experiences from which these principles have been derived and have taken a determinate form.

From this exposition, to which we shall refer from time to time in the following pages, it appears that up to a certain point we can distinguish the sum total of experience and reflection from which the science of statics has arisen, from that which has led to dynamics. The former has descended in part from the Greek

[1] *Op. cit.*, page 358.

geometers, and especially from Archimedes, and in modern times has been developed by the labor of Stevinus, Varignon, Galileo etc. down to Newton. The latter is an entirely modern science, the foundation of which is closely connected with the names of Galileo, Huyghens and Newton. As a transition from one to the other it seems to us that Descartes's attempt at a kinematic construction deserves mention.

The history of the progress of ideas gives us an instructive lesson by showing us how concepts are gradually extended to cover new cases, and by showing us how an experiment undertaken for a special purpose may have brought to pass the conscious recognition and fixation of certain combinations of sense data when these combinations themselves were partly due to a previous instinctive process of coordination of more familiar observations and experiments. Now when the human mind reaches this stage of recognition, it has the feeling of *discovering*, as if *a priori*, something general which another will receive as an *evident principle*.

Many now rightly insist upon this point that "the evidence of principles does not in any case constitute an *a priori* proof that shall be valid in the face of possible contrary experiences." But we have no need to go over again a question that has been sufficiently discussed under the head of geometry.

But, as it seems to us, we should be going too far in this direction if we begin to undervalue the *evidence of intuition*, by pointing out the *errors* to which it has led, without sufficiently taking account of the fact that these could have been corrected by means of a truer *interpretation*. With equal justice we could undervalue even direct experiment, since even that cannot be secured from the danger of false interpretation.

Now while there seems to be no doubt that the evidence of intuition must yield in case of a final conflict with the critically estimated answers furnished by experiment, yet we are glad to emphasize the fact that such conflicts never end with a complete condemnation of intuition, but rather that they have forced the latter to embrace a wider range of sense data, thus eliminating the apparent contradiction.

These reflections are well fitted to the ideas of mechanics. Those who depreciate intuition insist upon the fact that certain principles which we regard as self-evident, are in direct contradiction with the views of our predecessors. No argument seems more fitted than this to establish beyond doubt the fact that self-evidence

bears no relation to a necessity based upon strict mental laws, but rests only upon a *historical foundation*,—at least unless we should admit that the human mind has changed, an hypothesis that is not worth dwelling upon.

A convincing example, in the sense just mentioned, seems to be furnished by the *principle of inertia*, of which we are considering here only the geometrical part, namely, "the rectilinear motion of a material particle, upon which no forces are acting." If, as is to-day asserted, this principle has, in a sense hereafter to be defined, the self-evidence of *geometrical symmetry*, how could the Greeks have misunderstood it? How could they have thought that *natural* motions were circular?

As a fact, this contradiction can be interpreted in this way, that our idea and that of the Greeks do not refer to the same "mechanical space." In the series of successive associations and abstractions by means of which, as we have seen, the geometrical concept of "space" has been formed, we need to eliminate from the various "physiological spaces" those asymmetries that belong to the observer. Now the general point of view has been gained in geometry, by means of the optical conceptions, and by disregarding the systematic asymmetry which gravity introduces among the tactile muscular data. But in the synthetic development of the idea of space, when we take account of the "aspect of motion" on the earth's surface, another systematic asymmetry is introduced which belongs to the rotation of the earth, so that the first result of the association and abstraction of "mechanical physiological spaces" is a *geocentric conception*. To get beyond this stage, and reach a degree of abstraction corresponding to that of geometry, we must free ourselves from the geocentric point of view in considering the universe. And every one knows how the founders of dynamics have been led to this view, because the progress of astronomy has actually confirmed the system of Copernicus.

It is not amiss for our purpose to observe that according to the geocentric notion, the hypothesis of circular motion that was accepted by the Greeks, actually corresponds to a simplified view of astronomical facts, although the Greeks did seek to confirm it by asserting that the circle was more perfect than all other lines!

To any one who realizes the experimental origin of intuitive knowledge, it may seem absurd to give it a place of special privilege in the ordered realm of science. Our confidence in intuition, however we explain it psychologically, must contain an element of dis-

trust when compared with experiments voluntarily carried on and consciously criticized. But the matter appears in a different light when we reflect that our experiments and criticism, at least in an early stage of development, may easily be incomplete and one-sided, while our unconscious working-up of our sense data represents a far greater wealth of comparisons repeated in different combinations. For this reason intuitive knowledge is probably of greater value than experimental knowledge, *provided* that both *refer to the same class of relations.* But on the other hand, in marking out the field of applicability, and in the progressive enlargement of that field, voluntary experiment takes higher rank than intuition.

Therefore, while we cannot pretend to reduce all scientific knowledge to an intuitive basis, it is highly desirable that the results gained by experiment should further the adequate development of the intuitive ideas, so that the new facts may be added in with the great mass of older instinctive knowledge, by means of more extended concepts. The connection thus established forms a guarantee that there is no logical contradiction between the different portions of our knowledge, because such a contradiction would have hindered that process of indubitable association and abstraction which has actually been completed.

These reflections show us that the attempt so often renewed in the history of mechanics to found the science upon self-evident principles is not amiss.

The mere forcing of that evidence would be wrong, either by interpreting it through metaphysical arguments as an *a priori* necessity, or by extending it beyond its proper field.

The attempt to which we have referred leads especially to this result, that we distinguish a first *category of principles, which appear as the fundamental condition for an abstract theory of the data of mechanics. In these princples the data are combined in subordination to the concepts of space and of time*, and precisely from this consideration comes the *self-evidence* of these principles.

But these evident postulates, which are the premises of statics and kinematics, are not a sufficient foundation for dynamics. We must *add to them* hypotheses directly suggested by experiment, and only in part connected with certain intuitive ideas. These hypotheses in fine now contain *nothing self-evident.* Here especially whatever is arbitrary in our scientific construction, is explained by *historical development.* On the contrary a *psychological interpretation of the history of mechanics* cannot allow an equally indifferent deter-

mination of the first principles, however true it is that they have come to light in a partial and successive way in the course of the study of particular problems.

§ 10. FUNDAMENTAL CONCEPTS.

The development of mechanics brings to light, in addition to the concepts of space and time, the fundamental concepts of a material particle, force, motion, mass and linkages.

The concepts just named generally appear in connection with certain ideas, and when the subjective element has been eliminated they will retain a totality of relations which appear as *implicit assumptions.* The principles of mechanics, properly so called, establish among the concepts themselves certain relations which constitute the *explicit hypotheses of mechanics.*

These relations make possible a formal definition of any one of these concepts by means of the remaining ones. We thus have a reduction which some have carried so far as to lead to a *nominalistic interpretation* of mechanics.

§ 11. MATHEMATICAL NOMINALISM.

The tendency toward nominalism which is now seen in every region of theoretic physics deserves some special consideration.

This tendency is in a certain sense a danger that accompanies the advantages of a constantly extending application of mathematics, and its particular determining cause is an illegitimate extension of the formal standards of criticism, which seems to give rise to a *false conception of scientific accuracy.*

Strict accuracy in physics demands that our knowledge shall be supported by the greatest number of facts, controlled by the greatest variety of proofs, and that the relations stated shall always be accompanied by an example taken from experimental data, from which we may deduce the *degree of approximation* which permits us to regard them as valid.

A mathematical representation of physical reality fitted to satisfy these requirements, would be furnished by an *analysis* of approximation such as is the object of Robin's interesting reflections. But from different points of view it is useful and sometimes even necessary to proceed more quickly by means of a preliminary abstraction which disregards a certain group of empirical data, and states as *postulates* those relations which, in their simple and exact form, are supposed to represent *the most important part* of the relations

of real things. One thus leaves out of the whole theory the errors that arise from small and negligible differences.

A body of doctrine constituted in this way may attain a well-defined conceptual form, if its deductive development is rigidly carried out, without altering the content of the hypotheses. But on the contrary, in relation to reality, the theory developed still remains only an approximate representation. The postulates seem to be affected by an error which only fresh observations and experiments can gradually correct, by modifying the hypotheses that have been adopted.

Now at each stage of the scientific development, strict accuracy demands that these errors shall be as far as possible estimated and kept within a certain limit. But above all accuracy demands that they must not be hidden, and that the formal coherence of the theory must produce no illusion as to the approximate character of the knowledge it contains.

Can it be possible that the logical progress in the criticism of definitions and postulates will serve to remove or diminish errors of this sort?

The very question must seem absurd to any one who understands the significance of the abstraction that has been made. This has enabled us to divide the problem into two parts, one of which is hypothetically simple, and the other endlessly complex, although its elements exert less influence over actual prevision. The student of theories retains, therefore, full liberty to manipulate the hypothetical premises for his purposes of mathematical treatment, but he should not pretend that he is thus conferring upon the theory a stricter physical sense.

He may in any case reduce the number of the postulates which state these premises, and to do so is very easy. These postulates in fact appear in the general form of equations of quantities which are conceived as defined by means of certain processes of measurement. It would then suffice to assume such an equation as the *definition* of one of the quantities mentioned in place of the corresponding measurement. Thus instead of a series of experiments which are necessarily inexact, one substitutes a free act of convention which has a precise logical sense. Thus then we have eliminated from the theory an element of error, because we have eliminated the fact to which the error itself referred.

Such a procedure may be justified, in certain cases, in view of particular aims. For example we may be trying to find out what

changes must be introduced in a system of hypotheses taken in a certain order of subordination, where a given form that is capable of receiving a more precise content, is assumed as a fundamental premise. In this case a process of discrimination takes place by which a theory is constructed which means the less the more plastic it is.

But by merely narrowing the meaning we can never come to a perfect theory.

Let us try, indeed, to transform all the postulates into simple definitions. Behold! All error is eliminated, but also all real knowledge! Absolute formal rigor of theory reduces itself to nonsense!

From such a transformation we cannot believe that even a student of mathematical theory can draw profit.

For him there were only equations to be treated analytically, according to the exact rules of analysis. But he knew that these equations represented imperfectly verified hypotheses, and he has merely managed to hide those imperfections from his own eyes. His liberty to carry out mathematical developments remains as before, theoretically unconditioned, but he has lost sight of those motives which might direct his research to some physical end. The evils of such a method of procedure appear all the more serious, when one refrains from comparing theory and experience, in concrete verifications and applications.

For the lack of a previous criticism of what the theory itself really means, we run the risk of adopting a standard of *practical accuracy*, which is in strong contrast with the extremely cautious standards of *theoretical accuracy*; that is of accepting without any comparison the quantities that we have obtained in the calculations of any experimental determination whatever. In fine the danger is that we may interpret our theoretical construction in as many different and not clearly distinguished senses, as there are facts which at the moment seem to justify these interpretations.

Is this just the result desired by investigators who are seeking absolutely rigorous method?

Certainly not. But they seem not to perceive that this would be the inevitable consequence of the nominalism to which an incomplete view of science and a transcendental aspiration must lead.

In accordance with the standards which have resulted from the preceding critique we now propose to analyze the fundamental concepts of mechanics, bringing to light the various implicit and ex-

plicit hypotheses contained in them. The implicit hypotheses will especially concern us, since they appear often in the mental acquisition of those concepts as the condition of combining empirical data through the aid of abstract ideas.

§ 12. THE MATERIAL PARTICLE.

Although the first problems actually dealt with by statics have to do with relatively complicated *systems,* such as the *lever,* the *pulley,* the *inclined plane,* still in view of future developments it seems well to begin the study of mechanics by taking up *"material particles"* as an elementary case.

This fictitious construction is fully justified by its conformity with those principles for the simplification of scientific research which P. Volkmann calls principles of the isolating and superposing of phenomenal occurrences. It will be well then to pause to explain the concept of the "material particle."

But first we should note the fact that this fictitious construction could be eliminated. Indeed G. A. Maggi in his *Principii della teoria matematica del movimento dei corpi*[1] has shown how the whole mechanics of extended bodies can be established without regarding a body as a system of particles.

This result, which is of interest both philosophically and mathematically, deserves notice, even though from various points of view it seems to us better to keep a place for the idea of the material particle, especially for the following reasons:

1. In many investigations a body may be regarded as a "particle," without any error that lies within the limits of the physical accuracy required. And in proportion as these limits expand it seems that there will be always bodies, which in a certain order of phenomena, can be regarded as particles without any perceptible error. In these cases the requirement that we should get rid of the imperceptible error, by adopting a *mathematical* standard of accuracy, seems rather useless, when it leads, as it does here, to a more complicated system of ideas.

2. The notion of bodies as made up of elements or "material particles" is so largely employed in the development of physical theory that it could not be given up without a sacrifice. But the difficulties connected with the concept of a "material particle" cannot be passed over in silence. Under what conditions is it legitimate to treat a body as a material particle? The first condition that we

[1] Milano, 1896.

usually have in mind is that its dimensions shall be relatively *small in comparison with those whose relations we are considering.* According to this notion, a grain of sand, if we are studying motion on the earth's surface, and equally well a star, if we are studying the motion of heavenly bodies, are entitled to be regarded as particles.

But to the condition already mentioned another is added: that is, if the body in question is regarded as in motion around one of its own points, its position in this rotation must make no difference to the phenomena in question. For this reason, for example, a minute wave of polarized light, similar to an element of a surface, cannot be regarded as a particle in considering optical phenomena. We shall later see how important these considerations are for the criticism of certain fundamental concepts.

We shall now proceed to treat successively the statics and the dynamics of the *particle,* and later of *systems.*

§ 13. FORCE.

"Force is either the cause or the effect of motion."

Those who think that this judgment has the value of a *definition of force*, are responsible for the confusion of ideas which causes many to hold that force is a *metaphysical* concept, in the worst meaning of the word which is equivalent to "non-sense."

In fact the statement that one thing is the cause of something else, has no meaning unless we are trying to find the bond of invariable succession between two phenomena, that are already determined as such by means of sensations.

The existence of a force is a *physical fact* which is defined by its relation to the *muscular sensations of effort or pressure.* From this point of view there is nothing mysterious or metaphysical about force, any more than about motion or any other phenomenon, the actual definition of which is always reduced, in the last analysis, to a group of sensations that are produced under certain voluntarily arranged conditions.

As to those who would banish the idea of force from mechanics, if from any aspect they can justify their view by pointing out their aim of reducing the science of motion to its primitive data, they have no right to claim that the idea of force is less intelligible than any geometric or kinematic notion, or that its explanation involves greater difficulties. For all physical concepts are equally explained by pointing out their content of sense data.

In any case the attempts to eliminate the concept of force show the tendency to replace the *muscular explanation* of phenomena by an *optical explanation*, whether in a general sense, by means of the hypothesis of fictitious entities (cf. Chap. VI), or in the more restricted field of the mechanics of astronomy, in which the phenomena are directly presented to the eye.

In this field the muscular data do indeed appear as a supposition, at which some people laugh, pointing out the anthropomorphic nature of Newton's force of attraction. Perhaps the attraction between the earth and the moon, or between the earth and the sun takes for granted the muscular sensation of these bodies?

But, joking aside, there is nothing ridiculous in the supposition that a man on the earth might experience muscular sensations that correspond to lunar or solar attraction. We see the former in the tides, and it could perhaps be shown by delicate experiments that the latter modifies gravity. There is in fact a deviation of the plumb line which amounts to about 2″ in twenty-four hours, and which has been recently estimated by S. Newcomb and Von Sterneck, as due to solar attraction, in accordance with theoretical prevision. Note that even the supposition of the muscular sensations which would be experienced by a man carried away from the earth, retains some significance, although it is not practically verifiable.

In any case, there could be constructed a science of astronomical dynamics restricted to visual data, and rejecting the supposition of force. It will suffice to consider (as Mach has done) Newton's principle of action and reaction as a relation between the *accelerations* of moving particles of matter, where certain coefficients (masses) enter.

But though it may be interesting to obtain in this way a kinematic description of the movements of the heavenly bodies, independent of statics, still we should not forget that the value of this description is quite restricted, and that in particular it gives us a theory of gravitation in which the fall of bodies to the ground is separated from weight—a theory in which the experiments undertaken by Cavendish for the purposes of verification would find no place.

What shall we say of a view which thus mutilates our knowledge?

Now we really ought to note that Mach's study, to which we have referred, does not tend to substitute systematically for force a nominal definition, by regarding force, that is, as the product of

a mass by an acceleration. But Kirchhoff's mechanical construction is more in this direction. The result is that force as thus introduced, for example in the theory of elasticity, no longer expresses the sense content of what we picture to ourselves in this field as "tension."

While in relation to astronomy the optical treatment of mechanics could be in a certain way justified, the extension of this tendency to mechanics in general has no longer any foundation, if the prejudices about "force" are eliminated, as well as those reasons which explain nominalism in mathematics, though without really making it acceptable (cf. § 11).

The ideas thus far developed lead us to regard force as a primary concept of mechanics.

The original meaning of *force* is then settled as *possible muscular sensations* of effort or of pressure, *in relation to a given system of reference, and under certain conditions.*

This sense is made more precise through the use of appropriate instruments, and is *extended* by the supposition that goes with the concept of a *realm of forces.*

A field of forces is taken for *granted* (in relation to given bodies and phenomena that define it) as soon as we admit the possibility of perceiving a force acting upon a certain particle of matter which is carried to various positions in that field.

Now we ought to make the concept of force determinate, by analyzing the *data* of the sensations that are connected with it.

§ 14. GEOMETRICAL DATA OF FORCE.

The sensations belonging to a force determine:

1. A *direction* and a *point of application, that is a radius.*

2. An *intensive element* which we need to make more precise.

For this end we need above all to get the notion of "equal forces applied at different points and differently directed."

This notion first comes to us, even if in a somewhat inexact way, from the comparison of sensations of effort and pressure, as we may experience in different positions. The judgment of the equality of two forces which this comparison enables us to make, is hence rendered more precise by the association of the sensations we have mentioned with physical data, as, for example, with the stretching or contracting of a spring or of an elastic cord etc., provided that such tensions and pressures connected with certain moving bodies, shall be perceived to satisfy two fundamental properties:

1. To reproduce themselves unchanged (apart from special complications) when the body returns to its original form and position.

2. To agree among themselves in every position of the bodies with which they are connected, once they have thus agreed in one particular position.

The procedure by which we thus obtain an idea of "equal forces" seems indeed parallel to that by which the notion of "equal figures" is introduced into geometry, by virtue of the motion of solid bodies, which fulfils certain conditions of invariability in relation to the observer's organ of touch.

We should now note the fact that a force, however produced on this earth, can be substituted for a force of equal tension, exerted by a weight by means of a cord passed around a pulley. This substitution has the advantage of arranging the forces in a simple and determinate way, and of giving a simple and concrete image of the elements that belong to force.

In fact forces generated by means of weights are the first to which the founders of statics refer. But when the advancing needs of technique and the developments of the science had attracted attention to other forces, the principles already laid down immediately gained a natural extension.

To the concrete image already described we may add the means of measuring a force, and hence the determination of its intensive element by means of a number or of a segment.

Indeed it will suffice to consider the tensions exerted by means of a cord which is made to support 1, 2, 3.... *equal weights,* noticing that however we undertake such an estimate of forces (by means of various heavy substances etc.) we always find that "the multiples of equal forces are equal."

The experiments which thus give us the (static) measure of a force, form a particular case of more general experiments, which could be, ideally speaking, repeated even away from the earth.

"For several forces acting in the same direction upon a material point we can substitute for all their effects (of tension, pressure etc.) a single force which may be regarded as their *sum.*" And if we wish that the *static sum* of several equally directed forces should be represented by this *geometric* sum, it follows that "To every force there corresponds a *directed linear segment,* determinate in size as well as in position, excepting for a factor which depends upon the unit of measure."

§ 15. PRINCIPLES OF GEOMETRY IN STATICS.

A progressive idealization already gives, as we have seen, a simple *geometric notion* of a force acting upon a material particle. Now the first principles of statics are so evidently connected with this idea that it would be difficult to find in them anything more than a geometrical knowledge.

We are here concerned with certain *symmetries* between *forces in equilibrium* which until they were dealt with in the early problems of statics were implicitly used as notions already in our possession:

1. Equal and opposite forces acting upon a point 0 produce an equilibrium, or conversely.

2. If a material particle A exerts a force upon another material particle B, the former has for its line of action the straight line AB, and the point B exerts an equal opposite force upon A (*the principle of action and reaction*).

We may observe that these principles correspond to obvious and familiar experiences, which very often (as for the second one) refer to the case of tensions produced by cords or of pressures produced by solid bodies. But if we try mentally to take account of the foundation upon which these principles rest, the idea arises that, according to the principle of sufficient reason, they are deduced from the symmetry of representative figures; that is in the first case from the symmetry of the two forces meeting at a point 0 in a plane perpendicular to those forces, and in the second case from the symmetry of space in relation to the segment AB and to its middle point (independence of the rotations around the straight line AB and the invertibility of the segment).

It would be illusory to regard this as a *geometric demonstration* of the principles of statics to which we have referred. The observations and experiments upon forces and equilibrium, though they are quite close to those which have given rise to geometry, really involve something more which is not necessarily included in our acquired idea of physical space. In any case the content of the latter idea is capable of extension, and the reflections we have set forth show that it can be extended so as to render the statical data we have taken up subordinate to itself.

In other words, the principles of symmetry in statics already partly express the notion that "the geometrical constructions (directed linear segments) are adequate for the representation of

forces, in connection with the physical phenomena of equilibrium." In this form the actual content of these principles appears as a *condition for the possibility* of that idea with which their self-evidence is connected.

§ 16. THE COMPOSITION OF FORCES.

The sum of forces having the same direction is simply a case of the "composition of forces."

The general principle of the latter may be divided into two parts, one of which belongs to statics and the other to dynamics:

1. In the phenomena of equilibrium, several forces acting upon one point may be replaced by a single *resultant* and determinate force, which is *equivalent to the system.*

2. Systems of statically equivalent forces, applied at the same point, are also equivalent as to their dynamic effects (motion).

This second postulate stated by D'Alembert in his Dynamics of Systems, immediately constitutes in the case of the particle a hypothesis which ought to be brought out by criticism, but which does not properly fall under the head of statics as strictly understood.

The discovery of the principle of the composition of forces was reached historically, by the inductive method, through the study of particular cases. Stevinus reached this result on the basis of the composition of perpendicular forces, indirectly perceived. Later, that is in continuation of the dynamic constructions of Galileo and Newton, Varignon succeeded in more explicitly establishing the composition of forces by deducing it from the composition of motions, and treating statics as a particular case of dynamics.

In both these ways the result was reached in its concrete form (parallelogram of forces), and only after this discovery did it occur to D. Bernouilli and Foncenex to deduce the rule of composition from the abstract principle of the existence of the resultant and from the principles of symmetry. This method was perfected by D'Alembert and Poisson, and taken up again recently by Battaglini, Genocchi, Darboux, Siacci, Andrade, and carried through various interesting developments.

Mach observes that we are *not* here dealing with a *geometrical demonstration.* While agreeing with him on this point we cannot share the opinion that this way of treating the problem should be condemned as historically and psychologically meaningless. Though it is a fact that the concrete rule of the composition of forces was discovered first, and that this discovery was reached by simple ex-

periments, it would seem on the other hand that an implicit notion of the *existence of a resultant* must be presupposed as a guiding idea of those researches which lead to its determination. Indeed a simple observation shows us every day that if a particle is pulled upon by several threads, a pull must be exerted upon it in a determinate direction, in order that it shall be in equilibrium, exactly as if the particle were drawn in the opposite direction by a single thread. Now we have here an extremely simple experiment, which may in a certain sense be regarded as qualitative, when compared with the quantitative determination of the resultant. And for this reason we are inclined to attribute to this experiment the more serious value of proof.

Let us admit, as a physical hypothesis, the *postulate* that "a system of concomitant forces is statically equivalent to a single determinate force (resultant)." In order to estimate this force we need to know that:

1. The resultant can be determined by substituting for some of the component forces an equivalent system (the *associative* and *commutative property* of composition).

2. Systems of geometrically equal forces are equivalent, and hence give equal results.

These principles are merely so many physical hypotheses, but the latter are supported, like the principles of symmetry the use of which, indeed, recurs here, by the general supposition that "forces can be adequately represented, in all their statical relations, by the corresponding geometrical constructions." And upon this fact is founded the evidence of the special hypotheses, evidence which therefore implies a broader conceptual coordination of the various sense data.

To our way of thinking, the mind makes no mistake in having confidence in such evidence, since it admits the possibility of correcting experiments undertaken for the verification of the principle without seriously disturbing the general view of all the relations of statics, when deduced from the comparison of conscious and unconscious observations and experiments.

§ 17. THE FOUNDATION OF DYNAMICS.

The historical development of dynamics from Galileo to Huyghens and Newton, results essentially in two general principles: the equation of motion of a particle, and the dynamic principle of action and reaction.

Let us try to see how and in what sense these principles are determined in Newton's system-making, and what implicit hypotheses are connected with the concepts that they bind together.

The equation of motion of a material particle, contained in Newton's first two laws, states that "Forces are proportional to mass and to the acquired accelerations, and act in the same direction as the latter." Its vector formula is

$$f = m\omega.$$

We have thus indicated a quantitative determination of the causal relation between force and motion such as is shown by the most elementary experiments, and here there appears a coefficient m which depends upon the point.

It will be well to take account of the difficulties which had to be conquered in order to obtain the formula we have given.

First let us observe that the forces generally produced by the muscles, or by the coiling up of a spring etc., when they are not brought into equilibrium by any resistance, are of extremely short duration and become simply an *impulse,* which is directly measured by the *velocity* of the body thrown.

But when a continuous force is in question, gravity for example, the sense-manifestation of this force as a muscular effect, is accomplished in different ways in the case of a moving body, according to the velocity with which the hand accompanies (and retards) its motion. We should also add the influence of friction when anything is drawn over the ground, an influence which makes the pulling force seem perceptibly proportional to the velocity of the motion.

From all this we see that the first idea suggested by a roughly approximate empiricism, leads to Aristotle's principle that force is proportional to velocity. Only a very thorough analysis could have found out that force is in relation, not with velocity, but with the acceleration imparted.

Now how was this correction made?

This historical problem should be more thoroughly investigated. Did Galileo perceive that Aristotle's principle was false? It is certain that at least in his study of the fall of heavy bodies he implicitly considered a force (gravity) as *independent of motion,* and perceived this force to be proportional to acceleration.

But the question with which the development of the very concept of force is connected, is cleared up when we recall the dispute that arose between the schools of Descartes and of Leibniz, as to "whether

the velocity (v) or the square of the velocity should be taken as the measure of a force acting on a moving particle."

We now know that Descartes's proportion holds good if we are considering constant forces (f) acting during a certain time (t), while Leibniz's proportion holds good in case we have reference to forces acting along a certain path (s) of the moving body. The equations

$$ft = mv$$

and

$$fs = mv^2$$

have respectively been called "quantity of motion" and "effective force." The former measures the *impulse* of the force, the latter the *kinetic energy* (2E) acquired by the moving body or the *work* accomplished.

Differentiating the former of the two equations just given, we get Newton's fundamental equation

$$f = m(dv/dt) = m\omega;$$

differentiating the second we have another equation

$$f = (d\mathrm{E}/ds),$$

which also gives the dynamic measure of the force, and may be taken for a starting point in a Dynamics of Energy.

The fundamental equation of the motion of a material particle

$$f = m\omega,$$

sums up a series of facts which give it a physical sense of its own.

This sense should be determined by a suitable interpretation, which requires us to

1. Decide upon the standard of measure in relation to which motion and force are defined.

2. Define physically the coefficient m (mass). Galileo in studying gravity took "the earth" for a system of measure, and "weight" as a measure of mass. Newton, in passing over to astronomy, was obliged to modify such concepts, and seek to establish an absolutely general basis for them, by assuming: as a standard of reference a *space that is absolutely at rest,* and as mass a number belonging to a body taken as isolated from all others (product of the mass by the *density*). But he made good the deficiency in the definition of this number by carrying over from statics to dynamics the *principle of action and reaction,* assumed then as a *"relation between the accelerations"* of moving particles.

The mind of the systematizer of dynamics was dominated by

a transcendental point of view which led him to state his definitions and principles in a form that if strictly taken is in part meaningless.

But a new systematizing of principles results from Newton's work itself, if by careful criticism we try to discover the presupposed premises through an analysis of the positive developments of the doctrine, that have been generally accepted as indisputable facts until the present time.

We purpose, in the following paragraphs to institute such a criticism, taking up in turn:

1. The methods of determining motion, which Newton conceived as absolute, and for which the laws of dynamics hold good.

2. Mass, the postulates connected with it, and their relation to Newton's principle of action and reaction.

3. The fundamental laws that are expressed by the equation of motion of a material particle, and especially the twofold hypothesis that is superadded to it, traces of which remain in Newton's formulation.

Finally by means of this study we may come to some general conclusions as to the principles of dynamics.

§ 18. MOTION.

The geometric concept of motion is actually relative.

Given a group of bodies, we say that they move with respect to one another when their reciprocal distances vary with the passage of time. Such a definition, which is uniform for all the various bodies, does not help us in any way to determine which of the bodies under consideration is *really moving*, and which is *at rest*. Nay, the words italicized are without meaning unless they refer to something else.

The choice of a body unvarying in form which we decide to regard as fixed, is then the choice of a *system of reference* of motion, which from the geometric or kinematic point of view is simply conventional and arbitrary. The *sensation of the motion* of any object whatever corresponds first of all to a personal choice of this system of reference. The changes in the position of the objects are referred to ourselves. Nor should we get beyond this physiological conception of motion, if the aspect of the phenomena did not vary together with the sensation that we call *our own motion*.

By the association and abstraction of the different physiological representations of motion we are led to the physical notion which regards the earth as a system of reference. And this choice, in

so far as we are studying terrestrial phenomena, forces itself upon every one.

How long it took men to discover this relativity in the realm of astronomy! And what a struggle with a belief so firmly rooted in the minds of men that for the lack of reasons they undertook to uphold their belief by violence!

But at last a more thorough study of the movements taking place within the solar system forced upon every one the choice of a more suitable standard, the sun, which was regarded as fixed instead of the earth.

And yet the sun itself moves, having also, besides its rotation upon its own axis, a motion of translation in relation to the rest of the distant stars.

Now which of these ought to be regarded as fixed, when we find that they change their relative position, which fact we infer from the comparison of the older astronomical observations, and from the recent spectroscopic studies based upon Döppler's principle?

For the lack of a system of bodies which could be reasonably regarded as fixed rather than any others, it has been desirable to give to *absolute motion* a signification without reference to any bodies. *Space* being conceived as an invariable object of reference, motion was held to be a variation of the position of bodies *in relation to space.* Such was the transcendental conception as accepted by Newton.

However it is useless for us to confute this notion, since we have already seen in Chap. IV that there is no object corresponding to the word "space."

Moreover, with the progress of critical thought, no modern philosopher could attribute any meaning to absolute motion, as the term has just been defined.

But this definition reappears in a new form when we have postulated (as for example by Volkmann) an *ether* which fills the universe, and in relation to which motion is considered. It is plain that we must regard the parts of this ether as invariable in their position with respect to one another.

Can the postulate of the ether lead us in this way to attribute a positive sense to absolute motion?

The answer involves the discussion of certain questions of fact.

Plainly the word *ether* simply substituted for *empty space* means no more than the latter. But the case is different if we admit,

for example, a retarding action caused by the friction of the ether against the moving substance, thus virtually postulating, in place of the law of inertia, the tendency of bodies toward a certain degree of relative quiet. And now this certain degree of relative quiet is taken as a standard of reference for that motion to which some students had tried to attribute an absolute meaning.

Yet we can find no basis of fact for this hypothesis.

It seems more plausible to regard this "motion in relation to ether" as "motion in relation to light or to other electro-magnetic phenomena" thus connecting it with the class of facts to which *astronomical aberration* belongs.

We intend to return in Chapter VI to the questions that arise from the ideas just sketched. But we may say that up to the present moment in the aberration to which we have referred, and in similar phenomena, experience has brought out nothing more than the *motion of bodies in relation to each other.*

We now return to the relative motion of bodies.

Being unable to find anything fixed by comparing bodies taken individually, the idea naturally arose that *something fixed* might be derived *from their sum total.* Thus, for example, in the planetary system the choice of the center of gravity as a fixed point of reference does not seem arbitrary.

But presumably (if we admit the infinity of the astronomical universe) there is no *center of gravity of the system of all the celestial bodies,* that is to say a point which is the limit of the centers of gravity of the different partial systems of bodies, *independent of the order* in which those bodies are taken.

However if the astronomical universe does not give us *points* which we can reasonably regard as *fixed,* a simple observation enables us to determine *fixed or quasi fixed directions in relation to the sum total of the bodies.*

The following is the observation to which we allude:

While the improvement of optical means enables us to discover stars at *ever greater distances,* their *relative velocities,* so far as we can infer from the angular variations of the light rays and from Döppler's principle, *do not surpass certain comparatively small limits.*

Now if we grant this observation the value of a general *postulate,* it follows that: *the straight lines joining distant material particles* (stars) *tend as their distances increase to directions which remain invariable with respect to each other.*

It will seem natural that these limiting directions should be

regarded as *astronomically fixed* because they stand for an ideal system of reference in relation to the sum total of all the heavenly bodies. To be more exact, since in reality this limit is unattainable, we will say that the astronomical universe, in case we accept the postulate just stated, gives us a system of directions that are *more and more fixed* according as we succeed in carrying our observation to a greater distance.

The practical value of such determination is shown by the fact that the light rays carried from the earth to all the stars properly so-called, already lead to directions whose angles only vary perceptibly in *a series of years*, and thus afford a standard adequate to ordinary needs. A progressive correction may be introduced, when necessary, by means of a comparison of the angular variations, that is to say, by discarding the nearer stars which vary their orientation more rapidly than the others.

It is very noteworthy that the laws of Newton's dynamics lead on the other hand to the discovery of those fixed directions that have just been defined. And this gain fully compensates for Newton's unfortunate expression which admitted the concept of absolute motion as given *a priori*.

As we have said the equation of the motion of a particle of matter was at first implicitly considered by Galileo, in the case of gravity, and as *relative to the earth*. But a wider comparison of the data of astronomy leads to the correction of this law, by showing that it has a more precise value when applied to the directions of the stars. Now if we try at present to give a positive meaning to the hypothesis that Newton held implicitly, in conceiving absolute motion, it would seem that we should state the following postulate:

Dynamic laws are the more precisely verified in proportion as they refer to a system of axes the directions of which approach more nearly to directions that are astronomically fixed.

Thanks to the laws of dynamics we are able to give to the expression "fixed directions" another meaning which no longer depends directly upon astronomy, but which still remains *relative*. This may be done either by referring to the motion of a material particle, or to the motion of a *solid body*, in which case dynamics enables us to determine certain *invariable axes* and *planes*, that is, axes and planes which form *constant angles* with those that are determined in an analogous way by *another moving body*.

It is important to see more exactly that in all these determina-

tions we find only directions or positions of invariable planes, and *nothing more.*

For this purpose let us take as our point of departure Newton's law of motion, and separate the *systems of reference into two categories*:

1. systems for which the above law is valid;

2. systems for which the equation of motion is different and more complicated.

Any one whatever of the systems belonging to the first category (a) may be *conventionally* designated as *fixed,* and we may speak of *motion* (if we prefer even of *absolute* motion) in relation to a. But it is important to see what there is of an *arbitrary* character in such a convention, that is, in what way the systems that belong to the first category can move with respect to one another.

For this purpose, and keeping fast hold of our convention, let us suppose that we are in a closed cage P, which is moving in any manner whatever through celestial space. We now ask if and how suitable experiments upon the motion of the bodies within P can give us any indications as to the motion of the cage itself in relation to a. Let us imagine that we have brought instruments with us with which we can explore the interior of P, and determine directly and indirectly the forces acting on a material particle that is moving within the cage etc.

The forces determined by such exploration within P are usually regarded as *absolute forces,* that is forces independent of the motion of P, and then a mathematical analysis of the question leads to the result that:

The motion of the system P in relation to a may be determined by experiments within P leaving out of account a uniform translation.

That is to say: The standards of movement in the first category, in relation to which the laws of dynamics are satisfied, are relatively at rest, or they move in respect to one another with a *uniform translation.*

This conclusion is erroneous because all the possible experiments which bring to light the forces within P have a significance that is relative in part to the motion of P.

It will be well to explain more fully the value of our critique.

Any way whatever of verifying a force by means of the sense-perceptions gives us a knowledge of that force in its relation to the system to which the instrument regarded as fixed belongs, or else

shows us the differences between the forces which act upon the different particles in the field of observation. A force which acts equally, with the same intensity and in the same direction, upon all the particles in the field is imperceptible, and in relation to that field, in case we abstract from the surrounding bodies, may be regarded as non-existent.

It is true that when we are on a locomotive, we can feel it "bump" against any obstacle, but that is because the new force that opposes its motion acts only upon the vehicle that is carrying us, and not upon our persons.

We can also perceive gravity acting equally on bodies that are placed above the ground. But we perceive this because of the resistance of the ground itself which being upheld by lower strata is hindered from falling, and is thus, as it were, not subject to the same force.

But if we were shut up in a cage that was falling to the ground, we should feel the sensation of the lack of gravity to which we are accustomed. But our condition during the fall would be exactly like that of a man who should similarly find himself in a cage, beyond the sphere of terrestrial attraction; excepting that the blow against the earth would swiftly and terribly solve the doubts of our hypothetical celestial voyager who should attempt to study the motion of his prison!

Therefore, while we are inside a cage P, our experiences within it tell us nothing about the translations of P in their relation to a system α of the first type, which is regarded as fixed.

The case is quite different for the rotations of P.

In fact if P rotates around an axis, we can perceive within the field of motion (contrary to the fundamental postulate of Newton's dynamics which we shall take up in § 22) forces apparently non-positional which act upon moving bodies, and which correspond to the *composite centrifugal forces* of Coriolis's theorem. In a way that is practically simpler, but substantially equivalent, we can ascertain the rotary motion of P by means of experiments upon the motion of solid bodies, since we shall verify, for example, the fact that (1) the permanent axes of different gyroscopes all change their direction equally, thus forming constant angles with each other, and that (2) a force is needed to make them deviate. Consequently the positive conclusion of our analysis is that:

The notion of a *force relative to* a frame of reference, enables us to *determine, by internal experiments* interpreted according to the

principles of dynamics, *the motion* of the system with respect to *a except for an undetermined motion of translation.*

In other words: the systems of the first type, for which the laws of dynamics hold good are not of necessity relatively at rest, but may be moving, with respect to one another, with *any sort of translation* (uniform or non-uniform).

An illustration of what has just been said is given us by the experiments *upon the earth* which have shown that it *revolves upon itself.* I refer to the pendulum experiment of Foucault, and to the deviation toward the east of falling bodies, as predicted on the basis of Coriolis's theorem, and verified by Tadini (1796) and more perfectly by Reich (1831).

The *fact* contained in the statement "the earth revolves," apart from comparison with the stars, is just this: In the phenomena of motion on the earth, if they are analyzed exactly enough, we find the same characteristic behavior which a rough inspection shows us on a rotating body, for example on a ship which is describing a circle etc.

The statement that the earth revolves around the sun has theoretically a similar meaning, but it does not seem as if the experiments to which we have referred can be carried out with sufficient precision to bring to light the phenomena in question. If however we compare the motion of the moon around the earth to the fall of a heavy body, the perturbations of that motion must be interpreted in a similar way to the experiments of Tadini and Reich.

An indirect proof of the earth's rotation around the sun can be obtained upon the earth itself from experiments which measure the variation of gravity during the day (cf. § 13).

Our analysis shows that the dynamic concept of motion, which Newton took to be absolute, contains by implication a fact, that is, the agreement between various ways of determining a system of directions that form invariable angles.

The analysis of this concept ought especially to bring to light the fact to which we have referred, and this obtains clearly wherever we adopt as standards of direction *fixed astronomical directions,* and from these postulate (in the laws of inertia etc.) the relative fixity of other directions that are defined by the motion of bodies under given conditions.

We thus agree with the idea of Mach, who now finds much favor with the more positive sort of minds.

In the following paragraphs about "motion" the astronomical

determination of the standard of reference will be regarded as understood. But we shall have occasion to note that it is possible to separate *a more general part of dynamics, anterior to the law of inertia, which is valid when we are considering forces and motions in their relation to any standard of reference whatever.*

§ 19. MASS.

When we attempt to study the conditions that determine the motion of bodies, it seems natural to distinguish as far as possible the *internal* characters, so to speak, of the moving body, and its *external* relations with other bodies etc.

This distinction appears very plainly in the earlier cases in which we are dealing with motion started by muscular force or by the stretching of an elastic etc. We have then a relative independence between the two determining elements which coalesce in producing the phenomenon, that which we measure as effort etc. (the force) and that which we connect with the moving body, that is its *weight* or its *mass.*

Weight and mass at first appear as exactly the same thing, but Mach acutely explains how they may be distinguished by means of an experiment in which two weights hung upon a pulley constitute a system which is in equilibrium in any position, and which in any case cannot be set in motion without employing an effort proportional to the sum of the two weights.

Newton who had perceived in *mass* something distinct from weight, came to picture it as *quantity of matter* and undertook to explain the concept of mass by his Definitio 1: "Quantitas materiae est mensura eiusdem orta ex illius densitate et magnitudine conjunctim."

This definition has been generally criticised, because instead of mass it introduces a new and undefined concept, density, a concept just as obscure as the one it is used to explain.

It is however unjust to hold that for this reason, Newton's definition is entirely meaningless.

The idea contained in it may in fact be interpreted as a sum total of defining conditions, which serve to characterize an invariant of addition as related to a certain *group of physical transformations* of bodies (motions, compressions and dilatations).

Let us try to explain this idea.

We will begin by observing that the comparison of bodies leads us to picture them by means of abstract concepts, in relation to

which we can speak of *physically equal bodies,* and hence of *homogeneous* bodies. This abstraction denotes the recognition that bodies called "equal" may be substituted for each other in various classes of phenomena (physical, chemical etc.).

Now from the general concept of physical equivalence we can proceed to further abstractions that deal with certain classes of phenomena. Bodies that in respect to these qualities can be substituted one for another will possess quite a general relative equivalence or have, if you prefer, certain *equal qualities.*

A general way of advancing such processes of abstraction consists of studying *groups of operations or transformations* (physical, chemical etc.) *by means of which certain unequal bodies can be reduced to equality.*

Abstraction then leads to the definition of a relation of "equality" satisfying the transitive and symmetrical properties (Chap. III, § 14), provided that we are actually dealing with a *group* of transformations, in the sense that mathematicians give to the word "group," that is:

a. Together with the operations A, B, there enters into the group also that which is composed of the two operations successively performed (their *product*).

b. Together with the operation A the *inverse* operation enters into the group.

The simplest group among the transformations of bodies is the *group of motions,* and it is a part of the concept of physical equivalence that such equivalence is not changed by motion. This fact is expressed by saying that every physical equivalence is *invariable* for the group under consideration.

We can get another simple group of physical operations by dividing a body into parts and reuniting the fragments in different ways. This group leads to a more general *physical equivalence which is distinct from geometrical form.*

After this case comes the consideration of the group G which is made up of *motions, divisions* and *recompositions, compressions and dilatations* of bodies.

Newton's idea of mass as the "quantity of matter" leads us to try to define for every body a *positive number* which

1. shall have the same value for all bodies that are reducible by means of the transformations in group G;

2. which shall possess the property of being capable of addition or the distributive property in relation to the sum of the two bodies,

in such a manner that "when we reunite two bodies we shall have a new body whose mass shall be the sum of that of the component bodies."

This is precisely what we mean by saying that mass can be defined as an *invariant of addition of bodies in relation to the group* G which we have already sketched.

The *existence of such an invariant* implies a supposed fact which ought to be postulated.

The necessity of this postulate results from observing that the narrower group made up of motions and divisions and recompositions, admits of one and only one invariant of addition, namely *"volume."*

The postulate here required can be stated in the simplest way by referring to homogeneous bodies:

Let A and B be two physically equal homogeneous bodies, and let us operate on B by means of divisions and recompositions, compressions and dilatations. If after any cycle of operations whatever we get a homogeneous body containing a part equal to a part of A, this body has the same volume as A.

Let us limit ourselves for a moment to the consideration of a sum total of bodies whose parts can be reduced to equality by the transformations of group G. We may choose a homogeneous body as standard of reference A, and take for all its parts the *mass proportional to the volume,* and hence define the *density* of an elementary body B (regarded as homogeneous) as the inverse relation of its volume to that of a transformed element equal to a part of A. Thus mass is defined, according to Newton's definition, as the *product of the volume by the density.*

The process of abstraction which leads to defining mass according to Newton's account, does not apply, as we have seen, to the sum total of all bodies, but only to a restricted sum total of bodies, which can be reduced within group G.

But *this group* G *becomes extended* when, on the basis of the *atomic* representation, we imagine *chemically reducible* bodies as obtained by means of ideal divisions and recompositions (compressions and dilatations) of their parts. And this extended group G also admits of the invariant of addition, defined by the narrower group, because the postulate above stated holds good in the broadest sense.

This was exactly the discovery of Lavoisier, whose *principle of the conservation of matter* may be expressed as follows:

Let A and B be two physically equal homogeneous bodies, and let us experiment upon B by means of *internal* physico-chemical transformations. If after a series of transformations we get a homogeneous body containing a part equal to a part of A, this body will have once more the same volume as A.

We may then consider mass to be defined by a system of chemically reducible bodies, as the *invariant of addition of physico-chemical transformations.*

In the atomic theory this definition corresponds to the *number of elementary particles* in bodies.

Now this definition of mass may be extended to all bodies concerning which it is permissible to adopt the representative hypothesis of the *unity of matter,* an hypothesis whose positive sense consists:

1. In the possibility of enlarging the group of physico-chemical transformations by considering those hyper-chemical transformations, for which all the qualities of matter are reciprocally reducible.

2. In the extension of Lavoisier's principle to hyper-chemical transformations.

The question of hyper-chemical transformations has at present come into fresh prominence, since Ramsay obtained *helium* from the emanation of *radium,* and interpreted the experiment as a transformation affecting the atom itself. It is not impossible then that the progress of science may attribute a positive significance to the definition of mass that is based upon the representative hypothesis of the unity of matter aside from the practical difficulty of comparing the masses in this class of ideas.

To distinguish from these problematical hyper-chemical transformations, chemical transformations depend upon the irreducible properties of matter. In other words, to express the idea mathematically, they constitute an *intransitive group* in relation to the system of bodies.

Hence it is important to see that the invariant of addition in its relation to this group does not suffice to define determinately the mass of a body.

In fact we will now consider the chemical elements designated by

$$A_1 A_2 \ldots ;$$

each of these has belonging to it an invariant of addition, respectively

$$m_1 m_2 \ldots ,$$

which contains a factor of proportionality that can be determined at will by means of the choice of a unit of comparison. Now any expression whatever of the type

$$a_1 m_1 + a_2 m_2 + \ldots,$$

constitutes an invariant of addition for the composite bodies

$$A_1 + A_2 + \ldots,$$

in relation to the group of physico-chemical transformations.

The expression under consideration contains thus all possible invariants of addition. Hence mass enters into it in correspondence with a particular determination of the constants $a_1 a_2 \ldots$; but exactly this determination is not given by the chemical transformations. And joined with mass there remain indistinguishable from it, from this point of view, other invariants of addition, some of which have a notable meaning, and are therefore considered in physical chemistry, for example the volumetric characteristics considered with reference to the so-called 0^0 absolute, certain optical and calorimetric characteristics etc.

From what has been said it appears that the class of considerations and representations according to which we tend to define the mass of a body as an *internal* quality, that is independently of the influence that external bodies may exert upon its motion, this class of notions, I say, results only in an imperfect determination of this property.

The development of our analysis nevertheless leads us to see that:

There is a class of phenomena of motion (started by the muscles or by elastic pressure etc.) which may equally well take place in case we substitute for the moving body, regarded as a material particle, another body (particle) which can be chemically reduced to an *equal mass*; while the substitution of *unequal masses* would change the phenomenon.

Schematically we may regard these cases of motion as cases in which *motion is independent of the physico-chemical conditions of the moving body,* provided that later we bring back the perception of this independence to the exploration of the realm of force in which the motion takes place.

Now if we think we have directly defined the class of movements referred to, we may define as "equal" the masses of chemically reducible bodies, which are capable of being substituted in such motions.

And the important point is that *this definition may be extended to the case of any bodies whatever,* because the following *postulate about mass* is valid.

When the motion of a material particle A is independent of the physico-chemical conditions of the moving body, we can, without altering the motion, substitute for A a body made up of another kind of matter, taken in a determinate quantitative relation with A.

On the basis of this postulate the *masses* of chemically irreducible bodies are defined as *equal* which can be substituted in the motions under consideration, and we reach a *general definition of mass,* by getting rid of the arbitrary element remaining in it.

The postulate about mass says in substance that "motion is independent of the properties of the moving substance." But this independence is subordinated to certain conditions which have been considered in the statement, and which can be more clearly determined by the aid of the following considerations.

If we picture to ourselves the forces which at every moment are acting on moving bodies, we might suppose that these forces depend upon the properties of the substance of these bodies themselves or upon their physical state. But then we must admit that motion is independent of the kind of matter, inasmuch as the latter does not modify the forces under consideration.

The application of our postulate about the possibility of substitution is rendered more precise by this criterion. For example, a particle A is pulled upon by an elastic thread. In this case the force does not depend upon the nature of A, and hence we must admit that the motion would take place just the same if we should substitute for A another material particle B, taking such a quantity of the matter of which B is composed as will make a mass equal to A. Now the postulate should be interpreted in this sense, that the two masses, of A and B, when found by experiment to be equal, may be substituted for one another in any other phenomenon of motion whatever, under the conditions already stated. For example, a spring presses upon A, and here also the force does not depend upon the nature of A, hence the motion of A must take place just as the motion of B does, in case the latter body (instead of A) is subjected to the pressure of the same spring.

On the other hand, if A is an electrified sphere near another similar sphere, a force is then acting upon A which depends upon the state of the body itself. In this latter case the substitution of B for A alters the forces and hence also the corresponding motion.

§ 20. POSTULATE ABOUT MASS AND THE DYNAMIC PRINCIPLE OF ACTION AND REACTION.

From the foregoing analysis we get two methods of proceeding by abstraction to a definition of mass, by defining "equal masses":

1. Either we start from the consideration of those characteristics of the moving body which, in a relative sense, we have considered to be *internal,* that is to say, not depending upon those external conditions that determine motion.

2. Or we consider instead the various possible substitutions of bodies in motion, under various conditions, therefore seeking in some way an *external* definition of mass.

The former way does not lead to a completely determinate definition, but it brings to light an important fact. That is, a *relation between the physico-chemical transformations of matter and the law of motion.*

The second way, to which in any case we must resort in order to complete the definition of mass, leads to the recognition of a general fact, consistent with the agreement of various possible substitutions of equal masses in motion.

This fact which is identical with the postulate of mass as stated, may be regarded as contained in the equation of motion when we are undertaking the study of forces. But we may analyze the matter under a different aspect, and abstract it from force by seeking to eliminate whatever is superfluous in the definition of equal masses. To this end it will be well:

1. To fix our attention upon a certain class of phenomena of motion, and hence *to define,* by means of a fundamental experiment, *equal elementary masses.*

2. To *postulate* then the substitutibility of equal masses in other classes of phenomena, under the necessary conditions.

This analysis leads us to bring the concept of mass into relation with Newton's principle of action and reaction.

In order to define "equal masses" it is quite natural to compare bodies by means of elementary experiments into which they enter only in pairs, regarding a pair as isolated from the influence of external bodies when the latter are so very distant as to exert no perceptible influence.

Mach[1] has developed precisely such an idea, taking as his point of departure *Newton's principle of action and reaction.*

Thus we are led to analyze the significance of this principle.

And it will be helpful to observe at once that Newton saw in this principle a simple extension of the principle of statical symmetry (§ 15) by admitting implicitly that "forces, resulting from the actions of bodies upon each other, act upon the bodies themselves when in motion as when in equilibrium." That is to say, "they act as if propagating themselves instantaneously." If we admit this hypothesis, which does not at all share in the self-evidence of statical symmetry, the statical principle of action and reaction furnishes Newton's *dynamic principle,* which by virtue of the law of motion may be translated into a "*relation between the accelerations* of material particles in motion."

It is just in this sense, and quite independently of the concept of force that Mach assumes the dynamic principle of action and reaction.

Mach's analysis, which we will here complete by explicitly taking up supposition 3, leads us to express the postulates contained in the principle already stated, in the following way:

1. Two material particles forming an isolated pair have direct accelerations in opposite directions to the line that joins them.

2. If two material particles A, B successively and separately compared with a third C, take on equal (and opposite) accelerations, A and B also when compared as forming part of an isolated couple, will take on equal (and opposite) accelerations.

3. If we consider in turn the isolated pairs made up of the material particles

$$A, C, \quad B, C, \quad A + B, C,$$

the relation of the acceleration of C to that of $A + B$ is the sum of the relations of the accelerations of C to A and of C to B.

On the basis of the preceding postulates we may then state our definition thus: The *relation of the masses* of two material particles is the inverse relation of the accelerations that they acquire when they are compared in an isolated couple.

(It may be well to add that the physico-chemical transformations within the bodies compared may very well alter the accelerations of which we have spoken, but not their relations).

The sense of the expression "*equal masses*" especially follows

[1] *Op. cit.,* page 226. Cf. Vaschy, *Nouvelles Annales de Mathématiques,* 1895. Maggi, *op. cit* and *Enseignement Mathématique,* 1901.

from the preceding definition, and postulate 2 expresses the *transitive property* of the equivalence. Hence mass is defined *by abstraction*, and its *character of addition* is contained in postulate 3.

To whoever accepts unconditionally the dynamic principle of action and reaction, as expressed in postulates 1, 2, 3, the postulate about mass taken in its strictly mechanical sense, appears *equivalent* to this principle. And it then becomes indifferent what observations or experiments we choose as defining the "relation of mass"; such observations and experiments as may be obtained for example by referring to *gravitation* (Vaschy), to *shock* (Andrade) etc.

We should however bear in mind the conditions that a body should satisfy in order to be regarded as a material particle (§ 12), and we should note that these conditions are realized with difficulty when bodies *strike* together, in which case the form and the direction of the bodies striking against each other influence the phenomenon so as to lead to perceptible deviations from the theoretic law.

We now observe that the *equivalence* admitted by Mach, *between the dynamic principle of action and reaction* (postulates 1, 2, 3,) *and the postulate about mass, is related to Newton's systematizing of dynamics.*

Let us suppose that the dynamic principle of action and reaction, and more especially postulate 1, does not in general hold good, or at least that, in an appreciable degree of approximation, its validity is restricted to one class of cases. Then the definition of mass that depends upon this idea must refer to one of these cases, but the postulate about mass when understood according to § 19 will express also something in relation to those phenomena of motion for which condition 1 is not satisfied.

In this sense the postulate about mass appears to us more expressive than the dynamic principle of action and reaction. From this may be seen the value of the different representations which lead to the two concepts that Mach takes to be identical.

Finally we shall observe that, apart from accepting the general dynamic principle of action and reaction, the procedure indicated by Mach may also lead to a definition of masses, provided that we refer the ideal defining experiments to those cases in which we are dealing with bodies *in contact* or with connected particles becoming separated through the breaking of the bond formed by their position of equilibrium, and in case we assume the *dynamic*

principle of action and reaction in a restricted sense, such as appears in Hertz's treatise on Mechanics.

So also Volterra, in his lectures on mechanics at Pisa (1890), compares the accelerations acquired by two material particles rigidly connected so as to form an isolated couple, at the moment when their equilibrium is broken by the breaking of their connection.

We may observe that, in case we do not here wish to exclude the concept of force, the ideal experiment referred to brings back the comparison of the masses to that of the accelerations acquired by various material particles subjected to equal forces. We thus get back again to the fundamental law of motion, which (as we shall see) gives us the most natural foundation for the comparison of the masses of chemically irreducible bodies, in a development which follows as far as possible the view 1 that was described at the beginning of this section.

§ 21. THE FUNDAMENTAL LAWS OF MOTION.

The fundamental law of the motion of a material particle is condensed, as we have said, in the vector equation

$$fm = \omega.$$

Its significance can be determined without further trouble if we have defined the frames of reference with respect to which it is valid (§ 18) and the mass m (§ 20). If on the other hand the mass has not been first defined in a general way, the equation just given leads, as we shall see, to the completion of its definition.

Since we wish to undertake an analysis of the facts presupposed by the law of motion, it will be well first of all to recall the facts of the historical development which have led to it.

The acquisition of this law is, as we have said, the fruit of an induction, by which Galileo's law of the motion of heavy bodies was extended by Newton to the case of any variable forces whatever (a case to which numerous studies by Huyghens refer).

Newton's formulation is contained in the two following laws:

Lex. I. Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum, nisi quatenus a viribus impressis cogitur statum illum mutare.

Lex II. Mutationem motus esse proportionalem vi impressa, et fieri secundum lineam rectam qua vis illa imprimitur.

Lex I expresses that *principle of inertia* to which Mach ob-

serves that Galileo was led as to a limited case, by the study of the motion of heavy bodies on an inclined plane.

Lex II is interpreted as expressing by itself alone the general equation of motion

$$f = m\omega,$$

which in its mathematical expression includes through $f = o$, Lex I. Hence we have here, according to Mach, a defect of superfluity!

Our own view is a little different.

First of all we think that in order to understand the spirit of Newton's system we need not get rid of the wholly recent idea that the law of motion is a *dynamic definition of force.*

For Newton that law must have expressed a relation between two elements which he regarded as already known: on one side force, and on the other side the product of the mass by the acceleration.

But how could he regard force as known, unless from the point of view of statics?

If we admit that the force of which Newton spoke in Lex II can be defined according to statics, Lex II itself then takes on a more restricted meaning than the differential equation

$$f = m\omega;$$

in fact one of the arbitrary constants of the integral is determined *a priori, in* its relation to the system of reference. And in order to restore to this law all the generality with which it is later used, we need to add a new supposition, which Newton thought he adequately expressed in the law of inertia (Lex I).

Let us then interpret Newton's text in this sense: Lex II refers to incipient motion, Lex I to motion on which no forces are acting. The general law of motion ($f = m\omega$) follows if we sum up Newton's two laws, where in fact he implicitly assumes an *undeclared hypothesis* which we shall analyze in the following section.

We meet with the facts supposed by the *law of incipient motion,* wherever then we hold that force can be defined statically, in the equilibrium that precedes motion.

The result of this analysis is expressed in a system of postulates and definitions which we are just going to state, where we take mass as an invariant of addition of physico-chemical transformations, and therefore as defined only by the comparison of chemically homogeneous bodies.

Postulate I.—If a force acts upon a material particle which is

at rest, the latter, if not held back, begins to move in the direction of the force.

Postulate II.—The acceleration imparted to a given point is proportional to the static measure of the force.

Postulate III.—Equal forces impart to (chemically) homogeneous material particles of equal mass, equal accelerations.

Postulate IV.—If two forces f_1, f_2, acting separately upon two material particles M_1, M_2, impart to them an equal acceleration ω, a force equal to the sum $f_1 + f_2$, acting upon the particle $M_1 + M_2$ obtained by joining the two particles will still impart to them the same acceleration ω.

From these postulates we can build up the

Definition.—The *masses* of two heterogeneous material particles are called equal, if equal forces, acting upon them, impart to them equal accelerations.

The superfluous element contained in this definition is a consequence of postulate II: if two forces equal to f impart to the material particles M_1, M_2 the same acceleration ω, forces equal to $2f$ will impart to them the same acceleration 2ω etc.

Furthermore the concept of mass which is thus defined fulfils the requisites which we have already analyzed, since the additive character of the mass is contained in postulate IV.

To fill out the preceding postulates we need also to complete postulate II, by recalling Galileo's principle which we shall later see extended by D'Alembert in the dynamics of systems (§ 27). We will state this as

Postulate V.—Several forces applied to a material particle, produce the same acceleration (the resultant of the partial accelerations) as their static resultant.

It now becomes important to make the following observation:

Postulates I....V, *which express the fundamental law of incipient motion, hold good in case of* (incipient) *motion in relation to any system whatever* to which force and motion together are referred, even if we are dealing with axes whose directions vary in comparison with astronomically fixed directions.

In fact let α be a system of axes having astronomically fixed directions, β any system however variable with respect to α, P a particle in equilibrium with respect to β, which at a given moment begins to move with respect to β itself.

The law of incipient motion states that: if we refer to β the force acting upon P as well as the motion of P itself, the force is

(in the vectorial direction) proportional to the acceleration. Now this statement may be converted into the following: the force relative to β is proportional to the variation of acceleration estimated in relation to α.

And hence it follows, in the theorem of Coriolis, that the law is independent of the frame of reference.

Precisely in view of this independence Reech and Andrade[1] have conceived the idea of replacing the classic expression of the law of motion by the proportionality of the force to the variation of acceleration.

There are two differences between this formula and our own:

1. The first is that we restrict ourselves to the case of incipient motion, in which case only can we properly speak of force as defined according to the method of statics.

2. The second is that we understand the concept of force as *relative* to the same system to which we refer (incipient) motion, and therefore we do not need to change the classic expression of the law.

The first restriction is especially necessary for our purpose of bringing to light an undeclared hypothesis which comes into the progressive extension of the law of motion.

§ 22. THE PRINCIPLE OF INERTIA GENERALIZED.

We have pointed out that Newton completed the law of incipient motion (Lex II of Newton) by that of *inertia* (Lex I), but that the general law of motion made use of in Newton's dynamics, adds also an undeclared hypothesis.

Indeed the passage from the case of incipient motion to the general case, by means of the principle of the resultant, requires that the force acting upon a moving particle can be estimated as if the particle were momentarily checked, at rest.

This hypothesis of *positional forces* came to Newton's mind very naturally, because his dynamics (however they were invested with a universal and transcendental aspect) were in fact the extension of Galileo's terrestrial dynamics to the realm of astronomy. Just this extensive development brings before our eyes the manner in which Newton reached his general statement of principles, and the form in which he expressed them.

Now if we want to postulate explicitly what requires to be

[1] *Leçons de Mécanique physique,* Paris, 1898.

added to the law of incipient motion, in place of Lex I, we will state the following *generalized principle of inertia*:

At each moment the movement of a material particle takes place as if it were being moved by starting from a place of rest, provided that

1. the respective positions of external bodies (which exert a perceptible influence upon the phenomenon) do not through this supposed pause experience any modification;

2. an impulse equal to the velocity of the motion is added to the force acting upon the particle, a force which is measured statically when the particle is at rest.

It is scarcely necessary to point out that this law postulates a fact that is verifiable with sufficient approximation whenever the hypothesis is sufficiently verified.

Dynamics can also *correct* the application of the principle by taking account, for example, of the resistance of the medium in which a given body is moving. We need then to consider also the motion of the medium, or at least to infer from the latter the intervention of a new force, due, for example, to the compression of the fluid or to its inertia etc., which depends upon the velocity of the given moving body.

In this sense *forces depending upon velocity* are introduced into the equations of the classic dynamics so as to substitute in the motion of a partial system that which belongs to a larger system, in which the material particles are moved by the action of *positional forces.*

Two points remain to be cleared up, that is:

1. the relations of the usual law of inertia (Lex. I) with the generalized principle of inertia;

2. the relations of the latter with Newton's dynamic principle of action and reaction.

1. The customary law of inertia is a particular case of our principle which arises from supposing as given a realm of negative forces. Our principle is equivalent to the union of this particular law of inertia and the hypothesis of the positional forces.

We will also observe that in the particular law of inertia, two presuppositions can be distinguished: the conservation of direction in motion, and the conservation of velocity.

The former agrees with the geometric intuition as to motion, if we conceive the forces as "the action of bodies," since a particle acted upon by no forces then appears to us as a particle upon which

the possible changes of (distant) bodies have no influence, and hence the path of the motion appears to us as a line "which would remain unchanged while making the astronomical universe revolve around one of its elements," that is to say like a straight line.

The conservation of velocity is a principle which appears paradoxical when considered in relation to familiar experience. It represents an abstract induction, which was gained as we saw by Galileo in a particular case. To this result the hypothesis of positional forces when joined with the law of motion leads us by continuity.

2. The relations between the dynamic principle of action and reaction and the generalized principle of inertia, may be expressed as follows:

The dynamic principle of action and reaction, taken as a general relation between the accelerations of material particles in motion, is a consequence of the principle of statical symmetry joined with the generalized principle of inertia, provided that we regard the forces as "actions taking place between bodies."

The dynamic principle of action and reaction, in the restricted sense given to it by Hertz, immediately follows from the combination of the law of incipient motion and the principle of statical symmetry.

§ 23. SYNTHETIC VALUATION OF PRINCIPLES.

The analysis of the concepts and postulates of the mechanics of the particle, have led us to ennumerate the implicit and explicit suppositions of the science, which may now be arranged in the following order:

1. the postulates about space (Chap. IV) and about time (§§ 3–8);

2. the postulates of statics: principles of symmetry and the principle of the resultant (§§ 15, 16);

3. the principle of the conservation of matter (§ 19);

4. the law of incipient motion (Post. I–V, § 21);

5. the hypothesis upon which the construction of astronomically fixed directions is based (§ 18);

6. the generalized principle of inertia (§ 22).

Postulates 2 and 4 are valid for equilibrium and for motion in their relation to any frame of reference whatever. Only postulate 6 requires that the directions of the axes of reference should be astronomically fixed.

When we bear in mind this condition, and the way in which the postulates themselves are acquired, and their relations of subordination etc., it will now seem legitimate to arrange our *principles* in an *ordered realm*:

a. Postulates 1, 2, which rest directly upon intuition and are controlled by different direct experiments, ought to be regarded as the surest and most general suppositions in comparison with the remaining postulates.

b. Resting upon this static and kinematic basis we shall have postulates 3 and 4, which are not equally evident by intuition, but which are demonstrated by direct experience.

c. Postulate 6 (which up to a certain point is bound up with hypothesis 5), and in the same way Newton's principle of action and reaction which is derived from this postulate, will be taken in the third place as hypotheses that are no longer proven by direct experience (conscious or unconscious). These hypotheses are suggested by induction from the study of terrestrial and celestial gravitation, and they gain from this study an indirect verification that is subordinate to the acceptance of the previous postulates.

We will now state our preceding conclusions in a more suggestive form by saying that:

Postulates 1....5 *define a more general mechanics, which holds good with respect to any frame of reference whatever, which includes equilibrium and incipient motion.* In order that dynamics may lead to determinate previsions we should add to this system an hypothesis, which in the dynamics of astronomy is most fittingly expressed by the generalized principle of inertia. We could modify this hypothesis by assuming in its place some more complex law, as we shall be led to do in Chap. VI. In any case the generalized principle of inertia would express this law with an approximation that would be just so much greater according as the velocities considered are less, or to put it otherwise, in cases that are relatively near to incipient motion. And this is the case with the planetary system!

Certain difficulties would arise only in considering enormously greater velocities (v. Chap. VI).

§ 24. THE STATICS OF THE SYSTEMS: RIGID CONNECTIONS.

Until now we have referred to the mechanics of the material particle. But in many cases we must actually consider bodies whose

dimensions are not negligible, and so we cannot avoid a certain representation of the latter in the very definition of "forces acting upon a particle."

The form, the size, and in general the geometrical characteristics of bodies in equilibrium or in motion, are closely associated with certain perceptible elements which we designate as the *nature* of the body, rather than as the forces which act upon the body.

We can conceive the body as made up of material particles between which certain *internal* actions or internal forces are at work, and then the conception of the body is reduced to that of a "system of points and forces." We thus find the problem of equilibrium and of motion brought back to the principles of the mechanics of the particle. But the forces of which we speak are really a supposition which it is usually possible to determine by verification.

On the other hand it is easy to see that, in many familiar cases, certain geometrical characteristics remain constant independently of the forces applied (or they vary in a certain relation with those forces). We may then express our observation by placing hypothetical *rigid connections* between the particles of the body, which is then regarded as a "system bound by geometrical conditions."

Such a supposition should be taken merely as the ideal simplification of a fact that is ascertained *a posteriori,* it may be, for example of the fact that the respective distances between the particles of a solid body remain, within certain limits, approximately invariable.

If for the concept of rigid connections we try to substitute that theory indicated a moment ago of a system of internal forces, or if on the contrary, as we shall explain in Chapter VI, we undertake to regard all the forces as reactions of the rigid connections, the simultaneous consideration of forces and of connections imposes upon our representation a superabundance of conditions. But in that case it signifies that certain *results* (for example certain rigid connections) are taken in the place of certain unknown *data* (internal forces) in the determination of the phenomenon.

Postponing then to Chap. VI every question as to the possibility of reducing these primitive concepts, we shall in the following study treat forces and connections as elements which have a positive significance in relation to experience. And it is due to these forces and connections that it is possible to treat visible phenomena without extending our representation to an invisible world. We shall however find leisure to point out in several ways the necessity of

admitting certain relations between the two concepts, which indeed are grouped around the representations to which we have referred.

§ 25. THE LEVER AND THE INCLINED PLANE: THE PRINCIPLE OF STATIC MOMENTS.

The idea of these rigid connections may be said to be implied from the beginning of statics.

Indeed Archimedes and Stevinus were studying the equilibrium of such systems as the *lever* and the *inclined plane,* before they were led to consider the composition of forces applied to a particle.

The history of the foundation of statics points out to us the road that has been traveled by the discoverers of the fundamental principles of equilibrium, principles which are connected with the ideas of *static moments* and *virtual work.*

The equivalence of the statical moments as a condition of the equilibrium of the lever was discovered by Archimedes, who claimed to have deduced it from postulates relating to elementary cases of symmetry and asymmetry, that is to say, "equilibrium obtained by attaching equal weights to equal arms," and inequilibrium which comes from attaching unequal weights to equal arms, or equal weights to unequal arms."

Mach points out a necessary error in Archimedes's demonstration. And in fact his reasoning tacitly makes use of the hypothesis that "without changing the equilibrium, a weight attached to one arm of the lever can be divided into two equal parts which are equally removed from their point of attachment. Or that for equal and parallel forces applied perpendicularly to a rigid rod may be substituted a resultant which if applied to the center of the rod, is parallel to the component forces and equal to their sum."

Nevertheless it remains for us to judge whether the hypothesis implicitly postulated by Archimedes, because of intuitive evidence or because of the conditions under which it can be verified by experiment, does not represent a more credible thing than the theorem that "the equilibrium of the lever corresponds to the equivalence of the static moments." Now in this connection we cannot share the somewhat scornful judgment of Mach. We have no doubt that Archimedes's demonstration is truly instructive.

Its value, regarded from our point of view, increases if we keep in mind the developments relating to the *composition of parallel*

forces acting upon the particles of a rigid system, developments which proceed in a similar way to those of § 16.

A treatise by Foncenex, later corrected by D'Alembert and Laplace, in fact enables us to deduce a law for the composition of parallel forces applied in the same direction, which can be determined by means of a constant k using the following hypotheses:

1. The existence of a single resultant;
2. The associative property of the composition of forces;
3. The principle of inverse proportions.

The ordinary law, that "the resultant of two parallel forces acting in the same direction is equal to the *sum* of their components," is included in the preceding and more general law, and corresponds in the latter to the expression $k = \infty$.

Now Genocchi has shown that, in connection with Euclid's postulate about parallels, we can deduce precisely $k = \infty$; and he uses the following postulates of statics in his reasoning:

4. The motion of a force along the line of action;
5. The principle of the composition of concurrent forces, which in fact is here interpreted in a more general sense, since we are dealing with the substitution of the resultant (for the effects of the equilibrium) no longer for forces acting upon a free material particle, but for forces acting on a material particle which forms part of a rigid body.[1]

Thanks to these developments we gain the notion that the state of equilibrium of the lever, expressed by the principle of the static moments, apart from any direct verification provided for it by quantitative experiments, may be indirectly connected with experiments in some degree qualitative, which are gathered together in a synthetic representation of phenomena.

Also through other simple systems we can get, in a similar way, a demonstration of the principle of static moments as a condition of equilibrium.

Let us cite for example the condition of equilibrium of a heavy

[1] For the relations of this question to Euclid's postulate, cf. R. Bonola *La Geometria non-euclidea*, published by Zanichelli. [An English translation has been made by Prof. H. S. Carslaw of the University of Sydney and published by the Open Court Publishing Company, 1912.]

There can also be found here a sketch of the method of treating the problem which is due to Lagrange. Notwithstanding the interest and simplicity of this method, we really prefer those cited in the text, because in them no dynamic intuition is resorted to, and this seems more suitable to the spirit of the question. Lagrange on the other hand implicitly postulates that "rigid connections which permit the same movements can be substituted for the equilibrium."

body on an inclined plane, as deduced by Stevinus by referring to the postulate that "a closed homogeneous chain passed around a triangle, will be in equilibrium." Also this procedure appears all the more suggestive because even Mach's judgment is favorable to it.

§ 26. THE PRINCIPLE OF VIRTUAL WORK.

The principle of equilibrium based upon the comparison of the static moments forms the foundation of the statics of systems in an earlier phase of development than the knowledge of dynamics, in which the first explicit elementary experiments are immediately connected with the intuition of equilibrium itself, independently of the consideration of possible movements.

A later phase of development is marked by the use of dynamic considerations, and especially by the comparison of virtual work, that is to say of work accomplished by the system by means of small (theoretically infinitesimal) displacements such as are reconcilable with the rigid connections.

(Let us remember that the *work* performed by a force f through a displacement of a particle is called the product of the force by the projection of the displacement upon the direction of the force itself.)

An early observation of Stevinus as to the equilibrium of systems of pulleys, and a similar and more general observation of Galileo concerning the inclined plane, which was later perfected by Torricelli,[1] have led up to the principle of equilibrium that was recognized in its far wider meaning by Johann Bernoulli (1717), a principle that was later used by Lagrange as the basis of *analytical mechanics*.

The principle of virtual work or velocity may be stated as follows: The necessary and sufficient condition for the equilibrium of a connected system, subject to whatever forces, is that the work performed by the forces through a virtual displacement in the system shall be zero.

This principle expresses a very general supposition, which is however *relative to the nature of the connections* of the system, and it should be regarded (in agreement with the historical point of view) as an acquirement of knowledge that has been inductively

[1] Torricelli's principle says that the equilibrium of heavy bodies corresponds to the minimum (or to the maximum) of the height of the center of gravity. Lagrange has observed that the general principle which he calls virtual velocity can be reduced to this particular case.

obtained, by the use of a progressive extension of the very concept of the "rigid connections."

The classic demonstration that is now commonly given of this principle[1] shows plainly that its general validity for all the usual machines depends upon an analysis of the various kinds of connections that are combined in them.

The process of demonstration rests then upon the verification of the fact that the principle holds good for elementary cases: a free particle, a particle moving on a surface, connected particles, surfaces which turn one upon another etc. And such a verification is accomplished by means of a direct comparison of the experiments which relate to these cases, or by means of a comparison of the condition of equilibrium that is expressed by the annulling of the virtual work with other special conditions of equilibrium, which are supposed to be known from previous conscious or unconscious experiences that are recalled in an imaginary view of the phenomena.

Hence it appears that the principle of virtual work unites by means of a general supposition a sum total of facts, that are partly represented as evident, and gains from this sum total other facts which fall under the control of various explicit experiments.

It is important to insist on this point: The validity of the principle of virtual work, inasmuch as it is presumed to be deduced from the postulates of statics, and from certain elementary conditions of equilibrium of the simpler machines, is limited by the nature of the rigid connections.

But there is nothing to oppose an inductive development of the principle which denotes its application to cases that are not reducible to the type under examination. In this way the *supposition* contained in the principle itself is really *extended,* while it leaves to the consequences deduced the task of legitimizing its more comprehensive meaning.

As a notable example of such an extension we may quote the case by which we are led to take the concept of the invariability of distance in an infinitesimal sense. This is precisely what happens in the problems of equilibrium of *threads* and of *flexible* and *inextensible surfaces.* We find another extension in the problem of *hydrostatic equilibrium* in which we assume as data the *invariability of volume.*

[1] Cf. for example Appel's *Traité de mécanique rationelle,* Paris, *Gauthier* et Villars, 1893 (p. 225).

In a different sense we are extending the principle of virtual work when we deal with cases in which we do not really take the *forces* as *given,* but leaving aside all hypotheses as to the latter, we directly estimate the *work* that comes into play, as for example in the theory of *elasticity* (Chap. VI). We now approach in this way that development of the classic mechanics which has taken concrete form in the construction of kinetics (Chap. VI).

Finally it is important to point out two observations.

The first is that the principle of equilibrium expressed by the neutralizing of virtual work always refers to cases in which the connections themselves are geometrical relations that can be expressed by *equations.* If we consider more generally also those connections that are expressed by *non-equivalences* (unilateral connections) the principle itself ought also to be transformed, as we know, into a *non-equivalence.*

The second observation is that the principle of virtual work, as well as mechanics in general, refers to the case that is the limit to which we can *abstract from friction.*

We ought however to mention the recent attempt of Almansi to treat friction as a case of forces with rigid connections (partly unilateral) since a very interesting extension seems to result from this.

§ 27. DYNAMICS OF THE SYSTEMS: D'ALEMBERT'S PRINCIPLE.

An observation of Huyghens in his study of oscillatory motion, taken up again in other forms by Giacomo and afterward by Johann Bernoulli, has been promoted to the rank of a general principle by D'Alembert (1743).

A system of forces U acting upon a body made up of connected particles may be substituted for the effects of motion, by a system of forces V applied to the elements (particles) of the body, equal to the product of the masses of the latter (particles) by the accelerations actually generated by U.

The *equations of motion* of the body are therefore found to be expressed by the conditions of statical equivalence of the systems of forces U and V, that is, by the *conditions of equilibrium* of the system of the lost forces U — V.

We have already shown (§§ 16, 21) that this principle already contains a supposition of fact in the case of the material particle. We ought now to observe that *D'Alembert's general principle is a*

consequence of Galileo's postulate which relates to the dynamics of the particle, in which he accepts certain *implicit hypotheses* which pertain to the *concept of rigid connections.*

Indeed it is easy to arrive at the demonstration of D'Alembert's general principle once we have admitted that:

1. The rigid connections may be replaced at every moment by forces (reactions) acting upon the particles of the system, depending upon the forces applied.

2. The dynamic resultant of the forces acting upon each particle are equivalent at every moment to the static resultant (Galileo's postulate).

3. Applying to all the particles of the system simultaneously, forces equal and contrary to the resultants of the connections (and of the given forces) does not alter the reactions of the latter.

Hypothesis 3 in particular expresses a notion of the rigid connections and of their reactions, which are conceived as in opposition to changes of distances or to certain movements of the points of the system, and hence they remain invariable so long as these determining conditions persist.

D'Alembert's principle taken together with that of virtual work, has enabled Lagrange to express in the form of an equation the problem of the motion of a connected system, whenever the connections are expressed by equations and in fine are reduced to the elementary types under consideration.

The equations of motion of a system express in a precise mathematical form the fact that "the motion itself is determined by the knowledge of the forces applied, of the rigid connections, and of the positions and (initial) velocities of the particles of the system at a given moment." These equations include as a particular case the conditions of equilibrium.

In the deductive development of analytical mechanics it has been found useful to transform D'Alembert's principle, or better the theorem that is obtained from it, by making use of the principle of virtual work, into other *equivalent* forms which are often capable of being applied more rapidly.

Gauss's principle of least resistance and *Hamilton's principle,* are simply transformations of that theorem of D'Alembert and Lagrange to which we have referred. But it must be observed that the first principle has for Gauss a more general meaning, inasmuch as it includes also the case of unilateral connections.

§ 28. PRINCIPLES OF KINETIC ENERGY AND OF LEAST ACTION.

We here refer expressly to the case of the systems to which the theorem of D'Alembert and Lagrange applies.

Since this theorem leads up to the equations of motion, the motion itself can be determined once we have given the forces, the connections, and the positions and (initial) velocities of the particles of the system which may be assumed at will at any given moment.

From this it results that any other principle that determines the motion of such systems,

1. will be a logical consequence of D'Alembert and Lagrange's theorem;

2. or else it will imply some condition restricting the data (the forces and connections).

Gauss's principle already quoted, which also extends to a more general class of phenomena, and likewise Hamilton's principle, afford examples of the first case.

The interest of seeking for more restrictive principles corresponding to the second case, is connected with the view that "the *forces* and *connections* are not *arbitrary* elements of the dynamic phenomenon," and in particular that their reciprocal relations imply a limitation, the knowledge of which thus becomes necessary in order that we may proceed further with the concrete prediction of facts.

If for example it is permissible to extend Newton's astronomical conception of forces as "actions of material particles, taking place according to the dynamic principle of action and reaction" (central forces), and if we thence reduce the connections to the cases which can result from such forces (cf. Chap. VI), there will be introduced a restrictive condition which will enable us to deduce from D'Alembert and Lagrange's theorem some more comprehensive determining principle.

Actually the hypothesis of the central forces leads first of all to the establishment of a result of great physical importance, that is, the *principle of kinetic energy,* from which a simpler determination of motion later results.

Let us briefly tell the history of this principle.

In the first place Huyghens, in his study of the motion of pendulums, was led to observe that "the variation of the sum of the

kinetic energy of the particles of a system in motion is equal and opposed to that of the work accomplished by the forces." And this theorem extends to the general case to which Lagrange's equations refer.

But this is not all. The variation of the kinetic energy or of the work may often be estimated without a knowledge of the path traversed by the motion, this variation depending only on the *original* and *final configuration* of the system. The germ of this observation is to be found in Galileo, who noticed that the acquisition of kinetic energy by a falling body depends only upon the vertical height of the fall. Huyghens and Euler gave a more extended meaning to Galileo's observation.

Finally Daniel Bernoulli perceived that the observation itself has a general value in the hypothesis of central forces. In fact in such a case the forces admit a *potential,* that depends only upon the configuration of the system, and "the variation of the kinetic energy of the moving system is measured at each moment by that of the potential."

This latter is exactly the principle of kinetic energy which (as we shall see in Chap. VI) is the prelude to the principle of the "*conservation of energy.*"

For systems acted upon by forces which admit a potential, the principle of kinetic energy enables us to reduce the principles of Gauss and Hamilton to a simpler expression, which constitutes the so-called *principle of least action.*

The motion takes place in such a manner that the variation in the mean value of the kinetic energy of the system becomes less during any given increment of time.

Historically this principle precedes Gauss. It was first stated, in a somewhat vague form, by Maupertuis, and later more exactly by Euler. Lagrange observed its connection with Gauss's principle.

§ 29. VERIFICATION OF DYNAMICS.

It follows from the preceding paragraphs that a mental development from empirical data has succeeded in carrying forward ideal associations and abstractions from the concepts of space and time, by the construction of a science of statics and thence of a science of dynamics which includes statics itself as a special case.

This mental development may be regarded as a series of inductions, proceeding in part from unconscious experiences, and in part from experiments consciously carried out and coordinated with

the former in such a manner as to extend the imaginative view or intuition of facts.

The variety of these experiences and their intimate association in a general system of suppositions, by means of which they agree and control one another, constitutes a preliminary verification of the suppositions themselves. We thus obtain a guarantee of the value of these experiences, a guarantee of a very high degree of approximation, however legitimate it may be in this connection to discriminate between our principles by classifying them in an ordered realm (cf. § 23).

To gain a more precise and certain estimate of dynamics, we now need to subject the consequences that are deduced from that science to a verification.

The theorem of D'Alembert and Lagrange, which sums up the suppositions of dynamics taken in their entirety, expresses a general relation between certain empirical data: the motion of a body, the forces acting upon the particles of the body (that is to say the field of forces within which the body moves), the distribution of mass (density) of the body itself, and the connections between its particles.

The principles of mechanics, and especially of the mechanics of the particle, may be conceived as particular determinations of that relation, corresponding to hypothetically simplified conditions. And therefore the deduction of D'Alembert's and Lagrange's theorem from the principles to which we have referred signifies that the dynamic phenomenon is explained in its complexity, as the superposing of a certain number of elementary phenomena.

Now the verification of dynamics requires a series of concrete cases in which the supposed data (motion, forces etc.) may be judged by experience with a certain degree of approximation.

It is then especially needful that:

1. We should have a field of known forces, as for example a field of constant forces, such as terrestrial gravity etc.

2. We should be able to estimate the mass of the moving body, and its distribution (density).

3. We should be able to determine with sufficient precision the connections between the particles of the body, as is especially the case with *solid bodies*.

These conditions are sufficiently well fulfilled in two cases which fall precisely in the realm of the phenomena from the study of which dynamics arose:

a. In the motion of free solid bodies to the surface of the earth (the fall of solid bodies, the throwing of projectiles etc.), where however we meet with the difficulty of taking account of the *resistance of the air,* the precise estimate of which leads us to complete the moving system by adding to the solid body the surrounding fluid, that is to say, a system that is subject to connections that are more difficult to determine.

b. In the movements of the planetary system.

Here the complete system may be regarded as made up of solid bodies, and although we cannot know how their density varies, it is permissible to abstract from this knowledge by regarding the bodies themselves, now as particles, now as spheres, either homogeneous ellipsoids, or bodies whose density increases with a certain uniformity toward the center. In the principal questions of the astronomy of position the error inseparable from such hypotheses is negligible, and so it is sufficient to estimate in their entirety the masses and the forces which there come into play.

Now these latter determinations are partly connected with one another and result from a hypothetical extension of certain experiments: thus for instance from those of Cavendish, of Carlini, of Airy, and from the verification of the forces produced by the tides, measured by collation with astronomical observations, interplanetary attractions are proved in the most direct manner; from the experiments of Bessel on the pendulum, compared with astronomical observations, is proved that the measure of the mass is proportional to the attraction that it exercises etc. We should however note that the degree of accuracy of these terrestrial experiments seems usually to be less than that of astronomical observations, from which indeed, by the use of suitable comparisons, we ordinarily obtain a correction of the weight of masses.

However the method of verifying dynamics which the astronomers have tried, signifies this:

The verification of dynamics is subordinate to the acceptance of certain hypotheses of forces and masses, which are in turn directly verified by comparison of astronomical observations with certain terrestrial experiments in the lesser degree of approximation that belongs to the latter.

A more precise verification of dynamics is given by planetary astronomy, only in this sense: we can determine an ulterior approximation of masses and forces so as to represent the sum total of astronomical observations, and to establish previsions which are

found to be confirmed within the limits of the very highest degree of precision; that is to say, with an angular discrepancy of 15″, or of 1″ of time, for the motion of the moon in two and one-half centuries, and similarly with a maximum discrepancy of 8″ of angle, or of half of 1″ of time, for the motion of Mercury in a century (a displacement of 41″ from perihelion); for other planets this discrepancy is less than an angle of 2″, although it leads to appreciable errors in relation to the node of Venus and to the perihelion of Mars.[1]

In the face of such results our first and strongest feeling is one of surprise and delight at this triumph of science.

But whoever has unlimited faith in the progress of science will draw from this success new strength for the search for still more precise knowledge.

And indeed the discrepancies to which we have referred, although they are practically a minimal, are still appreciable by the use of our delicate instruments, and they surpass the limits of error that are predictable in the application of Newton's theory. They therefore require an adequate *explanation.*

We are now led to choose between the following hypothetical explanations:

1. We may correct the error by an extension of our observations (for example, by admitting the existence of new interplanetary masses, or the non-spherical form of the sun etc.)

2. We should for this reason modify the law of attraction, keeping fast hold of Newton's dynamics in its entirety, and hence assuming an attractive force $[f(r)]$ the function of the distance r, but no longer strictly proportional to $1:r^2$. In this latter sense Hall has proposed to substitute for 2, the exponent 2.000,000,151.

3. It is necessary suitably to modify the suppositions contained in Newton's dynamics, or at least a part of them, for example the instantaneous propagation of attraction and hence the generalized principle of inertia.

In dealing with a very high order of approximation it is hard to say whether, in conformity with hypothesis 2, a determination of the function $f(r)$ might not be possible which should confine the error within negligible limits. But such an hypothesis must surely seem very unsatisfactory without the support of some representations, and hence would give very little support to our confidence in dynamics.

[1] Cf. Tisserand, *Traité de Mécanique céleste,* t. IV, Chap. 28.

The idea of correcting the hypothesis of attraction, by admitting a finite time of propagation, occurred to Laplace, who however did not consider that this correction affected the principles of Newton's dynamics themselves and therefore was probably connected with some other correction of those principles. The calculation of Laplace, further carried out by Léhman Filhés (1884), showed a velocity of propagation millions of times greater than that of light. In fact it seems impossible to distinguish such a velocity from instantaneous propagation.[1]

At this point we will leave the astronomical verification of dynamics. The truly marvelous degree of approximation that has been reached, seems difficult to surpass in this field of observations, to which it will be necessary to return only after the comparison of still more ample experiments (cf. Chap. VI).

Beyond the two principal cases upon which we have already paused, it is helpful to consider what verification of dynamics is given us by other classes of more complex phenomena, and especially to see how such a verification is afforded by the workings of machinery.

Now we all know that the previsions of the theory require manifold corrections. The forces, masses, connections and motions which appear to us as the *visible* part of the fact, no longer suffice to determine it. Side by side with these things we must also consider the *elements of perturbation,* and first of all *friction,* with which are connected the phenomena of *heating, electrifying* etc. Hence we are forced to admit that in such cases dynamics represents a roughly approximate knowledge, and furthermore that the conditions we have mentioned exert a *systematic* influence, and hence cannot be neglected even in a *statistical theory.*

However this judgment will be modified in the direction of a more precise verification of dynamics if we succeed in *extending the sum total of visible data,* by taking account for example of the vibratory motions that appear in the case of sound etc.

Now this extension is theoretically unlimited, if we admit that by means of hypotheses we may pass beyond the field of experience. Side by side with the visible part of the phenomena, we shall thus be led to construct an *invisible* hypothetical world, which withdrawing always further from that which falls under the observation of the senses, will assume the significance of a *fictitious intermediary between real objects* (Chap. II).

[1] Tisserand, *op. cit.,* t. IV, Chap. 28.

Dynamics as thus extended may be said to be verified if the correspondence established between the fictitious entities and the physical reality leads to an adequate knowledge of the various relations between phenomena, and especially if it leads to a satisfactory correction of our previsions in respect to motion.

But such a verification must be in any case subordinate to the acceptance of other auxiliary hypotheses, which serve as a basis for the representation by means of which forces are estimated. We shall however, in Chapter VI, study the value which should be attributed to them.

We will now conclude our critique with the following observation:

In an extended interpretation of dynamics only certain combinations of the elementary data of this science will assume by definition the significance of real objects, and verification by experiment will bear directly on the relations of such objects, that is, on certain theorems deduced from the hypothetical principles, presumably not equivalent to the premises. Hence it would seem to be a natural idea to take the theorems themselves in place of the principles as the fundamental suppositions of a generalized theory.

In this way the physical interpretation of the deductive development of mechanics, may succeed, by the use of a series of successive inductions, in continuing the process of acquiring the concepts of science, in accordance with the opinions illustrated in Chapter III.

How this interpretation may further result in a true correction of the classic mechanics, we shall see in Chapter VI.

VI. THE EXTENSION OF MECHANICS.

A—PHYSICS AS AN EXTENSION OF MECHANICS.

§ 1. THE DEVELOPMENT OF THE PHILOSOPHY OF MECHANICS.

"*To explain and represent as motion, everything in which the fact of motion is continued, and everything that precedes it,*" such was the problem undertaken by Descartes's philosophy, and for two centuries the greatest theorists of physics have turned their attention to the solution of this problem.

Let us leave out of sight for the moment that which properly characterizes the thought of Descartes, in contradistinction to other similar tendencies, and let us consider in its more general aspect the genetic process of forming concepts, which results in the construction of mechanics, and in its further extension in physics.

We then recognize in this process a twofold development:

1. an *internal* development, which has for its guiding idea the progressive *reduction of the primary data* of the senses, associated in concepts;

2. an *external* development, more especially an *extensive* development, which aims to associate with these same concepts an ever widening variety of sense data.

The limits of both developments were boldly fixed by Descartes, even before the science of mechanics had been established by Newton. On the one hand the reduction was to be carried far enough to suppress every *qualitative difference* among the data, to reach, that is, a perfect unification. On the other hand the extension was not to meet with any limit, and the whole physical universe was to find its adequate explanation in mechanics!

This tremendous metaphysical plan represents, so to speak, the ideal scheme within which the progress of modern science moves.

The enormous work of two centuries has certainly brought notable additions to physics.

But at last, in our own times there has resulted from this work a consequence that was absolutely unforeseen by those who furthered it. I mean a new criticism of those very principles which were formerly regarded as rigid and indisputable truths,—an examination of the meaning which can, or which should be attributed to them, in order that they may be valid in a more extended class of relations, and finally an actual correction of Newton's dynamics, a correction which some more recent developments make plausible.

Such are the characteristic views of the thought of to-day—a thought which, while it recognizes the increasing value of science, rejects as absurd the pretensions of giving to mechanics *an absolutely rigid and universal value,* although it cannot fix a limit to its extension and to the corresponding perfecting of its principles.

The original thought that guided the philosophy of Descartes ought to be above all critically clarified in its tendency toward the reduction of qualitative differences to quantitative relations.

§ 2. QUANTITY AND QUALITY: THE CARTESIAN HYPOTHESIS.

"Quare opium facit dormire?
Quia est in eo virtus dormitiva,
Cuius est natura sensus assopire."

This celebrated answer of Molière's physician is often referred to as a type of the *scholastic explanation,* which indeed would quite lose its ridiculous aspect, if it were reduced, according to the Aristotelian spirit, to a simple *empirical* corroboration of the fact stated, and did not pretend to add anything to that fact by inventing the *name* of a corresponding *quality.*

As opposed to the Aristotelian view, we have the Cartesian concept of a *rational explanation,* based upon a metaphysical hypothesis. Beneath the various *qualities,* which are the appearances of the phenomenal world, there is a *single substance* which is only capable of *quantitative distinctions.* The explanation of phenomena is thus reduced to the knowledge of the relations of quantity that are inherent in them.

This contrast of the ways of understanding the matter of "explanation" has been illustrated by Duhem[1] in its historical aspect.

In our critique we wish to bring to light some other aspects

[1] "L'évolution de la mécanique," *Revue générale des sciences,* 1903, pp. 63, 119, 171, 247, 301, 352.

of the distinction between "quantity" and "quality," one of the most delicate distinctions with which the philosopher must busy himself.

The units or objects of a *class,* through which the *equivalence* and the *sum* are defined, we call *quantity.* A consequence of this definition is the possibility of establishing a *relation* between such objects (the *measure* of one in relation to the other).

To say that two units, within a given class, are equal, means that we "abstract from certain of their differential characters, in order to gather together their similar characters in a single abstract representation."

The actual meaning of the sum is the "re-uniting" or "putting together" one "beside" the other, or "after" the other, of the units taken as parts or addenda. That is to say, the thing that matters is the direct association, in space or time, of the groups of sensations corresponding to the parts summed up.

Let us refer to some examples.

§ 3. EXAMPLES: WEIGHTS.

Distances, volumes, weights, constitute equally classes of quantities. They are conceived as such so soon as we have acquired the notions of "equal distances," "equal volumes" and "equal weights," in relation to that of the "sum."

Let us pause upon the example of the weights.

Let us take a pair of scales or a steelyard, with a certain weight, and suppose that the body A placed on the pan is in equilibrium with the weight. Let us substitute the body B for A. If the equilibrium remains we say that "the weights of A and B are *equal with reference to the given scales.*"

The meaning of this definition is an association and an abstraction. We distribute the bodies in classes, placing in the same class those which may be substituted for one another when compared with the same weight on the scales or steelyard, and hence we frame the abstract concept of a "weight in relation to the balance," a concept according to which every element in the class may be substituted for every other element. The formal properties of equivalence (Chap. III, § 14) reflect this psychological process of abstraction.

We now make a second abstraction. We compare different scales or steelyards. "If two weights are equal when compared with one of these, they then are equal when compared with any

other whatever." We may then consider the equality of the weights as a *relation between* the bodies compared, *independent* of the reference to any particular scales.

We will now define the *sum*. The sum of the two weights A, B is the weight of the body obtained by uniting A and B, or of any other body of equal weight which can be substituted for this united body.

"Given A and C there always is a weight B which added to one of these, will give a weight equal to the other."

From this it follows that weights may be considered as quantities, the sums of parts.

Yet it is not evident *a priori* that "the sums of equal weights are equal." Indeed if A and A′ balance equally the scale with a certain weight, and so also B and B′, it does not follow that A′ could be substituted for A on the evenly balanced scales which have A + B on the scale pan.

But, given the variety of possible constructions, we perceive that this capability of substitution is implied in the weight being independent of the scales.

We have therefore the fundamental relations that enable us to regard weights as a "class of quantities."

We can frame the definition of this class in various other ways, and their agreement implies the validity of the facts which are usually presupposed in the concept of weight. Thus for example, by using a pair of scales (and it is here essential that we are not to use a steelyard), we can directly compare the weights of the two bodies A and B by placing them in the two pans. The first thing to observe then is that "the equilibrium remains unchanged when we exchange A and B," and hence that "if A balances B and C, B and C will also balance each other," and finally that the "equilibrium of A, B is independent of the particular pair of scales used as standard." Thus we have the fundamental properties that are inherent in the "equality of weight."

§ 4. QUANTITY OF HEAT.

A second example concerning the definition of classes of quantity is furnished by heat.

The "quantity of heat" lost or gained by a body in any change of temperature, may be defined by referring to the mass of a given calorimetric substance which varies according to the said loss or gain between two given temperatures (the equality of temperatures

is supposed to be defined on the basis of thermal equilibrium, as we shall presently explain).

This method of comparison leads directly to the settlement of what is meant by "equal" quantities of heat, and by "the sum of two quantities of heat."

We thus manage to express a process of abstraction which supposes three fundamental facts, analogous to those that we met with in the former case: the independence of the relation defined in respect of the particular calorimetric substance used for reference, the substitutibility of quantities of heat that are designated as equal in calorimetric comparisons (the transitive property of equivalence, Chap. III, § 14), and finally the equivalence of the sums of equal quantities.

The verification of the facts to which we have referred is the essential condition which makes that process possible which leads us to define quantity.

Thus in many cases this process becomes impossible. Heat itself gives us an example of this as soon as we try to define, no longer the quantity of heat, but the *temperatures.*

In this case we make use of a standard of comparison, the thermometer, beginning always with the phenomenon of the passage of heat between bodies in contact. We define "equal temperatures" on the basis of thermal equilibrium, and the relation is then independent of the thermometer, and satisfies the formal properties of equivalence.

But now let us consider the sum. By uniting two equally warm bodies we do not get a warmer body. The temperature does not vary.

We are thus checked in our attempt to consider temperatures as quantities, or the sums of parts.

To succeed in this purpose we should need to possess not the concept of "equal temperatures" but that of "equal increases of temperature."

We could then regard as the sum of two increases—from A to B and from B to C—the increase in temperature by which we pass from A to C, by uniting two successive passages.

But if every thermometer used for reference enables us to define equal increases of temperature, this definition is strictly relative to the choice of the substance in the thermometer, because "increases of temperature corresponding to equal expansion of a given substance will not cause the same expansion of another substance."

And on the other hand, in the increase of temperature there is no datum of sensation which could serve to decide the choice of a certain group of thermometers.

§ 5. THE MEASURE OF INTENSITY.

Now what is the significance of the metaphysical hypothesis which postulates beneath all variations of phenomena a quantitative substratum?

Let us suppose, for example, that heat is due to a particular fluid contained in bodies.

For an observer endowed with sufficiently acute senses, the process of heating would then be accompanied by the sight of an increase in the quantity of the fluid. Let us take the phenomenon of heat in its complex wholeness, the impressions of touch and the observations made by sight. By abstracting from the first class of sensations, we thus succeed in representing temperatures as quantities proportional to the density of the calorific fluid.

The hypothesis of the calorific fluid then has this value, that it leads us to regard a quantitative element as *essentially* associated with temperature. It is true that the perception of this is not granted to the obtuse nature of our senses, a fortunate circumstance which prevents us from endangering our metaphysical hypotheses. But we keep our confidence that amongst the various possible associations of the phenomena we may be able to discern a group of *notable* associations, in which an indirect corroboration of its hypothetical substratum may be perceived. Furthermore the idea of the calorific fluid leads us, as we shall see, to make a concrete choice.

The Cartesian hypothesis fulfils the same general office as to the *measure of intensity*.

Let there be established in a series of phenomena the sense of the *equal* and of the *more*, in such a way that we can obtain by abstraction a *series of degrees of intensity*. Now indeed these degrees may be associated with the numbers or with the increasing quantities of a class. The Cartesian hypothesis promotes a similar association, and in every one of its concrete expressions directs it in a sense which is not arbitrary.

We should here reflect that the confidence that we can find a measure of intensity, in relation to an essential aspect of phenomena, accomplishes meanwhile the effect of giving value to the use of a measure.

Even if the associations promoted have not an actual significance that is particularly notable, the fact that they induce us to measure is in itself an important result, a point which is thus appreciated by Duhem (*loc. cit.*, p. 301):

"Cette extension de la notion de mesure, cet emploi du nombre comme symbole d'une chose qui n'est pas quantitative, eût sans doute étonné et scandalisé les péripatéticiens de l'antiquité. Là est le progrès le plus certain, la conquête la plus durable que nous devions aux physiciens du XVIIe siècle et à leurs continuateurs;....ils ont établi cette vérité, d'un prix inestimable: *Il est possible de discourir des qualités physiques dans le langage de l'algèbre.*"

§ 6. NATURAL OR ABSOLUTE MEASURE: TEMPERATURE.

But as we have said, Descartes's metaphysical hypothesis not only leads to a quantitative idea or measure of intensity, but also it attempts to limit the almost boundless arbitrariness of the associations that are capable of furnishing it, and thus would give us a privileged, *natural* or *absolute* measure.

Let us go back to the example of temperature.

Its measure being relative to the thermometer is defined by means of a substitution

$$\tau = f(t),$$

in which f denotes an increasing arbitrary function.

Now the hypothesis of the calorific fluid, causes us to form the idea of an *absolute* measure of temperatures, or leads us to inquire "whether together with the variations which bodies undergo in the process of heating, we may find associated some quantity, which does not depend upon the quality of the body, and which therefore increases equally, with equal temperatures, in diverse bodies."

In fact if we make use of this hypothetical way of representing the facts, the quantity of heat Q contained by a body will be a quantity of fluid proportional to the volume occupied and to the density t, and t will represent the absolute temperature.

Hence: the hypothesis of the calorific fluid leads to the admission that the quantity of heat gained or lost by different bodies in passing from equal temperatures to equal temperatures is proportional.

This consequence is far from being verified in general, but it is verified in the *limiting case* of reversible transformations (adiabatic expansion or compression) in which the condition of the

indestructibility of heat, supposed by the hypothesis, is found to be realized.

And the important point is this, that the positive definition of *absolute temperature,* to which we are led in connection with the reversible transformations, becomes actually independent of the hypothesis of the calorific fluid. It remains as the expression of a fact, however inadequate the hypothesis to which we have referred proves to explain the conversion of heat into mechanical work, and the reciprocal production of heat by the friction or the percussion of bodies etc.

The fact presupposed in the definition of absolute temperature may be recognized in different representative hypotheses, as for example in the *kinetic theory of gases* (cf. § 10), in which the average kinetic energy (*vis viva*) of the gaseous molecules in motion is taken as the absolute temperature.

This fact is equally contained in both representations inasmuch as they express certain associations of the data of the senses. But they are not wholly identical and the second representation gives something more, that is it leads to the fixing of the *zero*; so that the temperature is defined by means of a factor depending upon the choice of the unit of measure, while in the first representation it is determined by means of a linear substitution

$$\tau = at + b.$$

And the signification of this choice of an absolute zero is the recognition of a *maximum* quantity of heat (or of work) which a gas can give.

Finally we notice that the fundamental fact that lies at the base of the definition of absolute temperature, is completed, thanks to the kinetic theory, by ascertaining that the quantities of heat taken up or given off by the various gases, are proportional to the variations of temperature, and hence are proportional to one another (Joule's law) and to their respective dilations. Thus it happens that we have an agreement between gas thermometers, which however holds good only in that degree of approximation in which the kinetic theory can be applied in its simplest form.

§7. RECAPITULATION AND CRITIQUE.

From the foregoing discussion we are able to draw the following conclusion:

The representation of a series of phenomenal data as quantities,

is the result of a certain process of association and abstraction, which presupposes facts.

The success of the procedure, in every group of associations, is interlinked with the existence of the facts indicated. But, in case the latter are not to be found, that success may be rendered possible by enlarging the associations themselves, that is by making other quantitative data which in reality are gathered together in the same group, correspond with the former phenomenal data.

However, the quantitative representation thus obtained remains an *arbitrary* convention, until we can determine a *group of noteworthy associations,* with regard to which the convention itself shall have an invariable character.

The metaphysical hypothesis expresses the general confidence in our ability to discover similar invariants (natural or absolute measures), and promotes a search for them as soon as it takes a concrete form in a suitable *system of images.*

But this is not all. The hypothesis to which we have referred enables us to consider the relations of phenomena around which our expectations are grouped, as capable of being expressed by means of *quantitative relations* between the (natural) measures of certain data. And the images adopted aid this *analytical translation* of physical reality, either by guiding us in ascertaining those relations to which we have referred, or by facilitating their interpretation. In this lies the value of the metaphysical hypothesis for the progress of science.

To tell the truth we have interpreted the metaphysics of quantity in this way, by emphasizing its positive content. But this metaphysics further claims that these images correspond to an *inaccessible and universal reality.*

It admits then first of all that its hypotheses are intended to represent facts, that is they imply *possible sensations for an observer endowed with sufficiently acute senses.* In the second place it admits that they *may* be concretely expressed in a way that is fit to give account of *all* possible relations between the most various classes of phenomena.

The first claim, for whoever should take it in a positive sense, would enormously limit the formation of mental images. A comparative examination of such images actually constructed, quickly shows us that these limits are not heeded at all; that the hypotheses mentioned change the nature of the imaginary sensations rather than magnify them; that reasoning from these hypotheses we are forced

to inhibit a series of contradictory consequences, from which we should not know how to escape in case we should attribute to them a content of fact (cf. Chap. I).

Remembering then that the metaphysical hypothesis represents in each of its concrete expressions a *model,* suitable to represent a certain class of real phenomena, it remains to be seen whether we can get hold of a single model that shall adequately represent *universal reality.*

The absurdity of this claim becomes manifest to whoever considers the *partial models* as the result of associations and abstractions, since a *universal model* would imply a transcendental extension of such a mental process.

Coming down to the examination of particulars we shall see still better how the construction of a model always represents a parallelism established between two series of phenomena, and hence has necessarily the value of a *relative* and *limited* reduction, inasmuch as no series can be taken as absolutely isolated from all others.

There would be still more to say about the *Cartesian substance,* deprived of any qualitative differences. But even if these objections were not manifest, of what use is it to confute a philosopher? Schopenhauer said that nothing could be easier, or more useless.

The metaphysics of quantity, though it cannot be accepted as such, still remains the guiding idea of a scientific movement which lasted a hundred years, and which we have explained in a positive sense as a mental process of association.

It is therefore important to see how this guiding idea has taken concrete form in a *science of physics considered as an extension of mechanics.*

We will then pass in review the theoretic developments, which through a dynamic view of physical phenomena, tend to fix the *quantitative relations* of certain data which are taken as characteristic, and to find in these relations the full determination of our expectations of actual facts.

And we here purpose to take up and explain:

1. The tendency toward reduction of the primitive data of mechanics, and its psychological significance;

2. in what way and up to what point, these developments lead towards the confirmation or correction of the postulates of mechanics;

3. the epistemological and heuristic value of the mechanical models.

§ 8. THE TWO TYPES OF MECHANISM: THE CARTESIAN AND THE NEWTONIAN.

The data of mechanics are qualitatively various: extensive geometric and kinematic data, and *intensive* dynamic data (forces). The internal development of the philosophy of mechanics tends to their gradual reduction.

According to the strictly Cartesian tendency we seek to reduce the intensive to the extensive data, through which the quantitative representation is obtained immediately, in connection with the sensations of sight. In this direction then we seek an *explanation of forces.* In just the opposite way, the Newtonian type accepts forces as elementary actions taking place between the particles of bodies, ranks them amongst the primary data, and attempts to unify them and to subordinate the explanation of other extensive data to them, in particular it attempts *to explain rigid connections* (solid bodies) by means of forces.

The two types of mechanism represent a difference of conceptions which can be illustrated in different ways:

1. The type of the Cartesian representation is an exclusively *optical model,* while the Newtonian type is a prevailingly *tactile-muscular* model of physical reality.

2. The Cartesian type corresponds to the *associative phase* of scientific procedure, while the Newtonian type corresponds rather to the *abstract phase.* Hence the quantitative representation of phenomena is for the Cartesian an acquisition of which we must explain the *genesis,* and of which visual associations give a *clear* and *distinct* intuition. On the contrary the representation under discussion is often taken by the Newtonians in a broader way, as an *actual* datum which can be utilized with profit according to its applications.

3. The Cartesian type especially corresponds to the *internal development* of the philosophy of mechanics, while the Newtonian aims rather at its *external development.*

Indeed the Cartesian tendency to polarize the representations in the optical direction should be interpreted above all as an effort towards a *more unified* association of the data of the senses, since the visual images have generally the most prominent place in mental constructions, and in any case they cannot be eliminated from our intuitions of forces, motion etc.

On the other hand, the Newtonian tendency towards a tactile-muscular model, which is superposed upon the visual intuition in an abtstract construction, should be regarded above all as an effort towards a *more extended* association of the data of the senses.

The former tendency has then a greater *economic* value for completed science, and a *psychological* value as a stimulus to research. The second is more helpful in the task of scientific *prevision.* But above all we must recognize that the success of the two tendencies depends upon their alternate use.

And thus the positive theory of knowledge reconciles strife by a more lofty view of scientific progress!

If we take for our higher aim the more unified and extended association of the various sense data, we shall in fact perceive that the alternation of those two developments has led us nearer to our aim.

To the first attempts made in the direction pointed out by Descartes we are especially indebted for the kinetic theory of gases. But the further development of the studies of elasticity (whether it be the search for a closer approximation to the representation of the properties of gases, to a representation of solids and liquids) this further development, I say, takes concrete form in an explanation of the Newtonian type. The latter gains a more complicated development in the mechanical theory of heat and attains its most abstract form in the mechanics of energy; in which however the views as to the localization and the motion of energy (in connection with the progress of electrical theories) strengthen the opposite tendency towards the Cartesian model.

A progress of the mechanical explanation in the Cartesian direction is in fact more plainly seen in the theories of optics, electricity and magnetism. The idea of rigid connections has been extended by Maxwell, W. Thomson, etc. The visual image of the atom occupies a more important place in recent developments, while however, the opposite tendency is reasserted in an electrical model of dynamics.

Although such a vast and varied movement of thought does not lead to one single conclusion, we may say synthetically that the developments pointed out have led on the one hand to a *unification of forces,* on the other hand to an *extension of connections*, and hence in various ways we have approached a more unified and extended association of the data of the senses.

Such views may be cleared up by an examination of the developments that have been carried on in the various fields of physics. But above all this examination will show us how the many partial acquirements justify a critical period of science, in which the lack of verification seems to arouse a new inductive evolution of mechanics.

§ 9. FORCES REDUCED TO IMPACTS: GRAVITATION.

Attempts to *explain* force were the order of the day in the seventeenth century. Under the impulse given by Gassendi, Descartes, and Huyghens the attempt was made to justify the dynamic properties of matter, by calling to mind the antique images of Democritus and Epicurus: *moving corpuscules,* their *impacts, the vortex motion* of surrounding liquids.

Let us notice especially, in this class of ideas, the hypothesis which makes universal gravitation result from the impact of *ultramundane corpuscles,* an hypothesis upon which Fatio de Duilliers has based his attempt at an explanation, an attempt later renewed by Lesage.

What is the signification of such an explanation of force?

We observe that elastic bodies move by striking against each other. The phenomenon consists of three moments: motion, force and motion. The force, escaping notice and appearing in turn as "effect" and as "cause," may theoretically be neglected by one who associates the two moments of motion, connecting them with the fact of the impact descriptively considered. We thus have a succession of visual images, whose relation, when once understood, would permit us to eliminate, and hence in a certain sense to explain, the datum "force."

The difficulty arises precisely from trying to determine this relation, without calling up the tactile-muscular image. But if on the other hand we seek aid from this, comprehending the elementary laws of impact by the consideration of the forces of elasticity, the result thus reached would plainly have only the relative value of a reduction.

In the hypotheses of Fatio and Lesage this reduction would be significant: the Newtonian forces acting at a distance, would be regarded as an effect of elementary forces exerted through contact (in agreement with the progress toward which the idea of cause

generally tends). And from it there would result in particular a correction of Newton's theory depending on "a finite velocity of propagation of gravity." Such an hypothesis has not taken on, up to to-day, a concrete form, in view of the observations of planetary motion (cf. Chap. V, § 29), but as we shall see, this hypothesis reappears at the present time in a new form.

§10. THE KINETIC THEORY OF GASES.

Daniel Bernoulli, in 1738, turned in the same way to the impact of moving particles, for an explanation of the pressure of gases. In this way arose the *kinetic theory of gases,* which was taken up again a century later by Krönig and Clausius (1856-1857) and carried to a greater degree of perfection by Maxwell, Boltzmann, and Van der Vaals.

According to Bernoulli's primitive account, the elementary particles of gas move in a straight line until they meet with the wall of the receptacle, and they all move with the same uniform velocity, from which arose naturally a measure of temperature. This view would need to be corrected if we wish to take account of the impacts amongst the molecules, a point which cannot be neglected in case we grant that molecules have dimensions other than zero. Through the effect of such impacts molecular velocities, even if they were supposed to be at first equal, must come to differ widely, and hence they should be taken into account.

The affair is then to represent the average effects of the phenomenon according to the laws of probability. In particular the average kinetic energy will provide the natural (or absolute) measure of temperature, in agreement with Joule's law.

The consequences of these premises are very noteworthy.[1] If we leave molecular dimensions out of account, we obtain as a *first approximation* the laws of gases expressed by the formula

$$pv = \mathrm{RT}$$

(p = pressure, v = specific volume, T = temperature, R = constant for all gases).

These are the laws of Boyle, Gay-Lussac, and Avogadro for the so-called *perfect gases.*

Let us add the important relation between the two specific

[1] Cf. L. Boltzmann, *Vorlesungen über Gastheorie,* Leipsic, 1896-98.

heats at constant volume and at constant pressure, (C_p, c_v) and the molecular weight (μ):

$$c_p - c_v = (R/\mu).$$

These laws are not exactly confirmed by experiments with actual gases. They differ in various ways, according to various conditions, and require to be explained by carrying out the theory to a *second degree of approximation.* And as Bernoulli foresaw, we already obtain a correction by taking account of molecular dimensions, but the latter is very far from satisfying the needs of experiment. Plainly a further correction should be sought by rising to the representation of the molecule itself, which has been compared to an elastic and impenetrable sphere.

This rather rough image ought to be modified. In what way?

The molecule already appears, in its chemical aspects, as a complex mechanical system, so much so that certain phenomena lead us to admit that there may be a dissociation into ions. However, in order to perfect the kinetic theory we make use of the hypothesis of a field of action that varies with the temperature, and of forces of repulsion exerted among the molecules within this field. Attention was thus fixed upon a particular law ($1/r^5$).

These developments, as every one can see, lead us far from the Cartesian mechanism, which according to the primitive notion was supposed to be capable of explaining the force generated by the pressure of a gas. But these developments mark an extensive progress of theory in the study of the properties of gases, although even the founders of the theory agree in attributing a provisory character to the hypotheses adopted.

§ 11. THE THEORY OF ELASTICITY: SOLID BODIES.

Let us suppose, however, that the simplest mechanism, founded upon the hypothesis of the impact of elastic spheres, should prove sufficient for the theoretic pursuit of the study of the properties of gases in agreement with the most precise experiments.

Nevertheless the elastic force of gases could not then be regarded as perfectly *explained,* unless we take for granted the notion of the *elasticity of solids.* As we have already pointed out, we must needs make use of the latter notion in order to explain the laws of impact, if we do not want to regard the latter abstractly as a mere optical fact, thereby cutting off an essential moment of the phenomenon.

We arrive at the same notion when we try to represent more

generally the physical nature of the connections by reducing them to forces acting between the particles of the body (Chap. V, § 24), and especially by trying to explain in this sense the approximate rigidity of solid bodies, the incompressibility of liquids, and the elastic reactions resulting from these.

The difficulty of framing such a representation of the solid and of the liquid state of matter seems to be greater than in the case of gases.

Let us sketch with broad strokes the history of this line of reasoning.

Inspired by Newton's astronomical conceptions, and by some of his views concerning capillary attraction, Boscovich (1763) thought out a general representation of matter as a mechanical system made up of particles endowed with mass, reciprocally exerting upon each other equal and contrary action and reaction, which, for every pair of particles, are directed along the line that joins them and depend upon their distance (central forces).

Such a theory was carried on, as far as the treatment of concrete problems, by the school of Laplace. The latter in particular brought to a high degree of completion the explanation of the phenomena of capillarity, considering these problems after the manner of Newton, as his various predecessors had done.

And this same tendency in the hands of Poisson accomplishes the construction of a doctrine which, under the name of physical mechanics, opposes the analytical mechanics of Lagrange, by attempting to reduce systematically the idea of connections to that of forces.

In 1821 Navier, in taking up the views of Poisson, for the first time subjected the principle of the theory of the elasticity of solid bodies to an analytical treatment. These principles were later developed, in various ways, by Cauchy, Poisson, Green and Lamé.

It will be well to examine more closely the conclusions reached by these studies.

Supposing that we start with the following hypotheses:

a. 1. Material particles constituting a solid body are fixed in a state of equilibrium.

2. The distribution of matter is homogeneous.

3. The internal forces result from elementary actions between the particles of the body, and are central.

We can now write the *equations of the equilibrium of elastic solids,* and among the latter 15 independent coefficients appear. If

the body is *isotropic*, the coefficients are reduced to a single one, and in particular we find that the coefficient of cubic compressibility and that of traction stand to each other in the relation 3:2.

Contrasted with these consequences of the views of Navier we have the results of the more general treatment according to the principle of virtual work (Chap. V, § 26).

Let us assume that:

b. 1. The particles of an elastic solid are subject not only to forces externally applied, and to the forces of mass, but also to *internal forces* as to whose origin we make no hypothesis.

2. The work of the internal forces depends upon the deformation of the element of volume, and hence, leaving out of account infinitesimals of a higher order, it is expressed in the form of a quadratic by means of 6 components of deformation, that is of the variations of the sides and angles of a parallelopipedon.

Now the principle of virtual work leads, in the case of Green, to more general equations of elasticity, in which there appear 21 independent coefficients in place of 15. In the case of isotropic solids we have *two* independent coefficients whose relation can have any value whatever.

Now the greater generality of this second theory really corresponds to possible cases. In fact while the coefficients of elasticity of metals agree in a certain measure with the previsions of the first theory, the case of a quasi-incompressible solid such as gelatine escapes those previsions altogether.

It then remains for us to make clear just how the hypothesis *a* ought to be modified.

For this purpose there has been proposed a modification of hypothesis 3 which is found even in some of the most authoritative recent treatises. It consists of the assumption that:

3′. The force acting upon a material particle of the solid depends *upon the totality* of the neighboring particles, and cannot be separated into a resultant of elementary actions. Or else that

3″. The elementary actions between the particles of the solid body are not central.

These hypotheses directly contradict the classic statics. The former denies the principle of the resultant, the latter the static principle of action and reaction!

It was natural to seek a way of escape from such a conclusion. Poisson already was obliged to attempt such an escape by making

use of hypotheses about the *form* of the atomic particles. But to tell the truth such considerations appear a trifle artificial.

W. Thomson has pointed out a more satisfactory solution, by proposing to modify hypothesis *a*2 or *a*1, especially the latter.

To assume that the particles of matter in a solid body in equilibrium are actually in motion, that is to say that where we see the body at rest we have a *statistical equilibrium,* in the sense of the kinetic theory of gases, is a way of solving the difficulty that might at first seem bold, but that is supported by the mechanical theory of heat.

If we accept this modification, the central forces that are acting between the moving particles can be replaced at each moment by equally central forces acting between the average positions. In this sense hypotheses 3′ and 3″ are explained as schematic simplifications of the representation.

The foregoing discussion brings out the way in which the hypothesis of a Newtonian mechanism enables us to explain the elasticity of solids, in the same sense as the principle of virtual work.

And what we have said about solids we may repeat concerning liquids.

Nevertheless we accuse this kinetic *theory of matter* of being in general not very fruitful of positive consequences, although the representation it gives us is connected with the development of the mechanical theory of heat.

We will look at this matter a little later. We will now examine an objection to the general theory of elasticity, which experiment has brought to light.

§ 12. PERMANENT ALTERATIONS.

The fundamental hypothesis which enables us to calculate the work of the internal forces of an elastic solid by an infinitesimal deformation, consists in granting that that work depends for every element upon the *actual local* deformation, and not upon that of the remaining parts of the solid, nor upon the series of *preceding configurations* through which the solid itself may have passed.

An obvious consequence of these hypotheses is that when the deforming force ceases the elastic solid must resume its original (natural) state, and in general that sudden elastic deformations ought not to modify its elasticity.

Now these conclusions are directly contradicted by the *permanent alterations,* which by analogy with the phenomena that Ewing

calls magnetic hysteresis, we also call phenomena of elastic hysteresis. And we are here dealing with a contradiction which affects the very bases of the mechanical theory of elasticity.

G. Robin in his lectures on general thermo-dynamics insisted upon the fact that the principles of mechanics contain an hypothesis of *non-heredity,* since in them it is postulated that the future state of the system depends upon *actual* static conditions. And in certain suggestive pages of his most recent book on *La science moderne et son état actuel*[1] E. Picard develops some interesting ideas of a form of mechanics in which, on the contrary, we must take account of *heredity.* Certain *functional equations* would there take the place of the classic *differential equations.*

The permanent elastic alterations, and in general the various phenomena of hysteresis, seem to require some such radical change.

But it is not impossible that we are dealing only with cases of *apparent heredity.*

In order to bring these latter cases back to the classic mechanics, it in fact suffices to suppose that we have not taken account of *all* the noteworthy elements that determine the phenomenon. That is to say, it suffices to consider the system as *part of a larger system,* to which the postulates of mechanics apply.

This way of solving the difficulty would give rise to many thoughts. But for the moment let us leave these sporadic cases on one side, to be taken up later, in the final conclusions of our critique.

And from the kinetic theory of matter we now pass on to the developments of thermodynamics that are connected with it.

§ 13. THE MECHANICAL THEORY OF HEAT: CONSERVATION OF ENERGY.

If, as Descartes in his time assumed, heat is an irregular motion of the elementary particles of matter, and motion can be explained by means of central forces, these must be a *mechanical equivalent of heat.* This consequence is implicitly contained in the principle of kinetic energy (Chap. V, § 28). It precedes by a century the discovery of the *conservation of energy.*

The credit of this discovery has been disputed by various investigators, and with the question of priority is connected the problem, far more interesting to the philosopher, of discovering the connections between theoretic speculation, and actual accomplishment by means of experiment.

[1] Paris, Flammarion.

Now without entering into an over minute scrutiny, we will say that:

The idea of the *calorific fluid,* by which Black's and Crawford's first researches into the subject of caloric were inspired, is to be regarded as the fundamental cause by which the discovery of the equivalence between heat and work was so long delayed in following the principle of kinetic energy.

The idea to which we have referred was directly disproved by the experiments of Rumford and Davy, who was again led in 1812 to consider heat as a form of motion. However the studies of thermal machines undertaken by Sadi Carnot in 1824 still regard the idea of caloric as indestructible. Only a few years later Carnot rectified this opinion and succeeded in finding a mechanical equivalent of heat. But his notes on this subject were not published for forty years after his death, which took place in 1832.

Between 1830 and 1840 various thinkers approached the idea that heat is motion, and got from that idea the view of permanence of what we now call energy. We will quote as an example a work of Mohr (1837) which was corroborated by Tait.

From this it follows that the mechanical representation of heat must be recognized more or less explicitly as a guiding idea in the search for an equivalence between heat and work, and hence must be regarded as preparing the way for the discovery connected with the names of Mayer (1842), Joule and Colding (1843).

Without discussing the respective merits of these three discoverers, it suffices to say that, whatever were their personal opinions, they found themselves confronted by a question already stated. Precisely in such a statement of the question we perceive the influence of the theory over the gaining of knowledge by experiment, that is to say the heuristic value of the model which makes heat a form of motion.

Against this thesis it would be in vain to bring up the personal opinions of Mayer which refuted such a mechanical conception. So much the more that in the researches of Mayer himself we already find postulated *a priori* the permanence of a something that is common to heat and to work, and, without demonstrating the latter fact, we obtain the numerical determination of the thermo-dynamic equivalent by the comparison of the specific heats of air.

The relation of the principle of equivalence with the mechanical theory of heat appears to be clearly understood by Joule and Colding, and it is shown in a still brighter light in the works of

Helmholtz (1847). If we consider every isolated system as a Newtonian mechanism, in such a way that there is supposed to exist a kinetic or a potential energy (whose local variation measures the force), we could consider the *total energy* of the system, that is the sum of that potential energy and of the kinetic energy as decomposed into different *energies* having a physical significance of their own. Thus we should have an apparent mechanical energy (kinetic and potential), a thermic energy or quantity of heat, and in general other energies also (electric, photometric etc.). The sum of the various energies named remains constant.

This is the general principle of the *conservation of energy,* which especially includes thermo-dynamic equivalence.

Let us explicitly state that the principle of the conservation of energy should at present be regarded as an *experimental principle,* independently of the hypothesis of central forces.

Such a statement does not contradict the recognized connection between the hypothesis and the principle. The latter is a consequence of the former, but not *vice versa.* Hence if various experiments which agree directly verify the conservation of energy, we then place the validity of the principle itself upon a new basis, while the original hypothesis which suggested the discovery receives only an indirect and incomplete confirmation.

Now the unanimous confidence in the agreement of the principle of energy rests upon two classes of proof:

1. The direct experiments of Joule, Hirn etc. in which we find the mechanical equivalent of heat appearing through the most varied transformations of energy.

2. The indirect proof of Helmholtz, who deduced the principle of the conservation of energy from the *impossibility of perpetual motion.* He considered that this impossibility was brought to light by a critical examination of the unfruitful attempts that have been made in connection with that problem.

§ 14. THE SECOND PRINCIPLE OF THERMODYNAMICS.

The second principle of thermodynamics *does not* in fact trace its origin to the mechanical representation of heat, but it *was* suggested by the analogy of the "fall of heat" with the motion of a fluid. We thus see the difficulties that appear in trying to reconcile this principle with mechanism.

This discovery is connected with Sadi Carnot's studies of thermic machines. It was necessary only to rectify the results obtained

by Carnot in the hypothesis about the calorific fluid, by bringing those results into harmony with the principle of equivalence between heat and work. This was done by Clausius in 1850.

The fundamental postulate of Clausius is that "heat cannot pass from a colder to a warmer body, without the employment of work and without other heat passing contemporaneously from a third to a fourth body at a lower temperature." From this postulate there is deduced an equivalent principle which expresses the impossibility of the so-called *perpetual motion of the second variety.* That is to say, "it is not possible to transform heat into work, by taking heat from one sole source at a uniform temperature."

By combining this principle with that of equivalence, we get the *theorem of Carnot-Clausius,* which we state in its most general form relative to reversible cycles. omitting certain restrictions of secondary importance which he uses in his demonstration.

Suppose we have a system of bodies which, after a closed cycle of reversible transformations, returns to its original state. We calculate, along the line of transformation *s,* the quantity of heat Q and the (absolute) temperature T of the system, and we then have

$$\int_s \frac{dQ}{T} = 0.$$ *

In consequence, if for an open cycle of reversible transformations we estimate the integral of Clausius $s = \int (dQ/T)$, we get a quantity that depends solely upon the state of the system and not upon the series of transformations that have taken place. The latter quantity takes the name of *entropy.*

The theorem of Carnot-Clausius may be assumed as the second principle of thermodynamics, and we must now try to see in what way it is possible to give it a mechanical explanation.

Let us form the hypothesis that heat is the perceptible expression of the (invisible) internal motions of the molecules. In the thermo-dynamic transformations of a body we shall then see a mechanical system in which we can distinguish two parts of which the total energy is composed:

1. The *internal* energy, representing the *quantity of heat* Q.

2. The *external* or *apparent* kinetic energy, which is due to the visible motion of the system.

But this second energy may be regarded as negligible in com-

* If the temperature is not uniform we must separate the system into so many elementary parts and consider the sum of the integrals of Clausius. Cf. Poincaré, *Thermodynamique.* Carrè, Paris, 1892 (p. 224).

parison with the former, if we are here limited to the consideration of *slow transformations,* in which the velocities of the visible motion are extremely small as compared with the molecular velocities.

In order to interpret mechanically the second principle of thermo-dynamics it will be necessary to indicate the dynamic expression of a quantity representing the temperature T, which for every elementary transformation shall be an integral divisor of the variation dQ.

Furthermore if, according to the point of view of the kinetic theory of gases, we consider thermic equilibrium as a *statistical equilibrium*, the fundamental property of temperature must be verified. That is to say by uniting two systems corresponding in statistical equilibrium with the same value of T, we should obtain a system equally in statistical equilibrium, to which the same T corresponds.

The representation of gases which is derived from the kinetic theory (§ 10) quickly leads to the definition of (absolute) temperature T in a manner that fulfils the conditions stated. That is, it is a consequence of the fact that the kinetic hypothesis contains the *laws of perfect gases.*

The mechanical representation of liquids and of solids does not agree so readily with the second principle of thermo-dynamics.

The principal attempts at the construction of a mechanism capable of imitating such bodies, are due to Boltzmann (1866), Clausius (1871), H. Helmholtz[1] (1884) and J. W. Gibbs[2] (1902).

These attempts, especially the more notable ones of the two latter authors, show us the possibility of the desired mechanical explanation, although they do not give a completely satisfactory answer to the problem.

In the mechanisms constructed by Helmholtz (the *monocyclic systems*) where hidden motions of a permanent kind take place, for example vertical or vibratory motions, T is defined, as proportional to the mean molecular kinetic energy, so that for every elementary transformation there will be an integral divisor of the variation dQ, but it is not plainly shown how the second fundamental property above recorded can be satisfied by T.

Gibbs has been especially zealous in fulfilling this condition and has succeeded by the use of the most general hypotheses, and by considering systems composed of an immense number of extremely

[1] *Wissenschaftliche Abhandlungen,* Bd. III, p. 176. Cf. H. Poincaré, *Thermodynamique,* p. 392.

[2] *Elementary Principles in Statistical Mechanics,* New York and London. Cf. T. Duhem, *loc. cit.*

varied elements (bodies). Unfortunately these systems depart rather widely from the Newtonian type of mechanism, in that they involve forces emanating from fixed centers, instead of the reciprocal action of parts in motion.

In Gibbs's systems, one particular condition (canonical distribution) being fulfilled, we can then define a certain quantity (modulus of distribution) T, which possesses the fundamental properties we have already pointed out. In order that the uniting of the two systems of canonical distribution in statistical equilibrium may give place to a canonical system in statistical equilibrium, it is both necessary and sufficient that the two given systems shall have the same modulus of distribution. Yet the equations of statistical equilibrium in Gibbs's systems, do not exactly agree with those of thermo-dynamics. There is a discrepancy, which however tends to become less with the increase of the number of parameters upon which the determination of every element in the system depends, so that *the laws of thermodynamics here appear as limiting cases of mechanics.*

§ 15. IRREVERSIBLE PHENOMENA.

It would be hazardous to try to draw any conclusion whatever from this result. Plainly the systems thought out by Gibbs are not the only possible mechanical illustration of the laws of thermo-dynamics. It does not then seem to be excluded that, even without departing from Newton's type of mechanism, we may be able to accomplish the construction of a somewhat general mechanical model which will strictly fulfil the desired conditions.

But there is another aspect of the problem which should claim our attention.

We have thus far considered only reversible thermo-dynamic phenomena. Now the latter is only a limit of the general case in which we have irreversible phenomena, for which the theorem of Carnot-Clausius should be modified by substituting a non-equivalence for the equivalence which was considered above. If we now define the variation of entropy[1] in this case also, we get the following result:

In every irreversible transformation of an isolated system, the entropy constantly increases.

This tendency seems then to give to the phenomena of nature a definite direction.

[1] Cf. Poincaré, op. cit., p. 229.

How can this fact be made to agree with the mechanical representation?

One of the most obvious consequences of the form peculiar to the equations of Lagrange is the reversibility of motion. Then any mechanism, in so far as it is governed by such equations as are in agreement with the principles of dynamics, must fail to give an example of irreversible phenomena.

This difficulty seems insurmountable at first sight. Nevertheless two explanations have been proposed for disposing of this paradox.

Helmholtz observed that only *complete* mechanical systems were reversible. *Incomplete* systems were apparently irreversible. Thus it was for example with Foucault's pendulum, by reason of the movements of the earth which complete the system.

Now then we may grant that the irreversible phenomena given us by experiment, are only a visible part of the complete phenomena of which *hidden movements* form a part. Irreversibility would thus constitute the appearance of facts which in their totality would be reversible.

We should not have any illusions as to the bearing of such an explanation. Duhem justly observes that it takes account of the existence of irreversible processes, but does not tell us why these all follow one common tendency, why side by side with apparently isolated systems in which the entropy increases there are no systems to be found in which the opposite takes place.

The explanation of irreversibility proposed by the founders of the kinetic theory of gases is far more satisfactory.

Let us consider a system composed of an immense number of elements that are moving in all possible ways, for example that system which we have pointed out as the model of a gas. A transformation of the system corresponds to the passage from one state to another, each state being defined by the velocities of the elements in size and direction.

For the lack of a precise knowledge of the process, it is impossible to say what would be the transformation of the system after a time t. But, in the sum total of all possible transformations, those appear as *more probable* which approach a distribution of velocities indicated by Maxwell, for whom a certain function H (which thus would have the office of entropy) becomes greatest, corresponding to a state of complete *disorganization* of the system.

It is then permissible to state that after a sufficiently long time

t, the transformation of the system will very probably take place in the direction of the increase of H. It is true that such a transformation would always be theoretically invertible. But the inverse transformation ought not to be regarded as equally probable with the one given, because its continuation through a value greater than t, would probably lead to a disorganized state in which H would have a greater value.

Let us dwell upon the delicate points of this process of reasoning.

Although the possible transformations of our system appear in pairs, each one being placed beside its inverse, it does not for this reason follow that every one of these would be just as probable as the inverse. This consequence would be legitimate only in case those transformations were finite in number, while they are infinite. Irreversibility thus appears as an effect of averages, in a series of phenomena that are individually reversible. From this side there is no objection to be brought forward.

Yet we must bring out the character of the explanation thus obtained. It adds to the mechanical representation of things the experimental principle which demands that the law of probabilities shall be verified in a great number of cases.

This principle supposes, furthermore, an equivalence of conditions in the development of the combined elementary phenomena among which the result of the average is sought.

Now who can assure us that a first arrangement of our mechanism could not change this equivalence and hence make our law inapplicable? Boltzmann (*op. cit.*) agrees as to this point that the theory is founded on an hypothesis. We believe that a more precise determination of it is desirable.

In any case we cannot deny the value of an explanation which proves capable of aiding in the thorough study of concrete cases, by constructing for example a kinetic theory of the diffusion of gases, and so illustrating mechanically an irreversible phenomenon.

Furthermore we have no cause for complaint that the mechanical theory of irreversible transformations shows us the possibility, under special conditions, of an overthrow of the natural course of phenomena, in thus contradicting the second principle of thermodynamics. Who assures us that there may not be any exceptions to the postulate of Clausius? For example, in the phenomena of life?

Maxwell's conception of a daemon as a distributor of energy

here comes naturally to mind. By choosing in a gas the molecules which have a greater velocity, and separating them from the others, the daemon could raise the temperature of one part of it, causing a loss in other parts, and yet producing no work.

But such a phenomenon would seem necessarily to require a *power of choice.*

Rejecting the use of such means, G. Lippmann[1] proposed an ideal experiment by which according to the hypotheses of the kinetic theory one might succeed in obtaining heat from a gas at a uniform temperature, placed in a magnetic field, thus contradicting the postulate of Clausius. The question is of profiting from the alternating induction currents which are generated by the motion of the electric charges carried by the molecules, taking account of the slight differences in the distribution of the velocities.

To tell the truth, these effects could not be made perceptible, and therefore they would not actually contradict the principle of thermo-dynamics, taken in its actual acceptation, as a fact of experience. But unless we are mistaken, another observation may be brought forward in opposition to Lippmann's line of reasoning: A gas at a *uniform* temperature always permits, according to the hypotheses of the kinetic theory, extremely minute differences of temperature at different points. It would seem then that such differences, progressively reduced, could be utilized as an infinitesimal fall of temperature, to produce effects of the same sort, in agreement with the second principle of thermo-dynamics.

§ 16. THE MECHANICS OF ENERGY.

Let us consider the preceding developments concerning elasticity and heat as an attempt at a more extended verification of the Newtonian mechanics.

The possibility of the verification depends upon the hypothesis of *hidden movements,* an hypothesis which we shall see assuming a wider extension in the electro-magnetic theory, in which *hidden masses* will also be postulated.

Now this hypothetical construction of an invisible world, as compared with the facts of experience which it undertakes to explain as consequences of the principles adopted, includes an arbitrary element that is so much the more unlimited, according as the irreversible phenomena oblige us to multiply the hidden movements,

[1] *Rapports du Congrès international de Physique.* Paris, 1900, Vol. I.

and to consider only the results of means according to the law of greatest numbers.

Considered under this aspect physics assumes the character of a *limit* in relation to mechanics. And the verification of the latter is definitively lacking, because the approach is opened for *possible modifications of the principles of mechanics,* which are of such a nature as to be eliminated in a calculation of the mean.

The preceding conclusion may be expressed by saying that the mechanical explanation of physical phenomena contains *indifferent hypotheses* relating to the consequences which constitute the general principles of the phenomena themselves.

Therefore by the tendency to *eliminate the indifferent,* a further inductive development of dynamics is justified, in which the principles to which we have referred are taken as more general premises in place of the Newtonian principles. We meet precisely this development in the construction of a *mechanics of energy,* which was originated by Helmholtz.

Suppose we should start with a Newtonian mechanism, that is, with a system of moving particles amongst which central forces intervene. We then have defined a *potential energy* and a *kinetic energy,* which, according to the various mechanical representations, assume a direct physical significance by being translated into measurable quantities. And now we can state the two following fundamental postulates:

1. The sum of the two kinds of energy in any isolated system is constant (conservation of energy).

2. The variation occurs in such a way that the *mean* of the differences between the one and the other kind of energy, for every interval of time, is minimal (*Hamilton's principle*).

The greater extension of these postulates as compared with classic mechanics, results from the following observations:

a. Every class of phenomena in which it is possible to define two distinct kinds of energy as kinetic and potential, so that conditions 1, 2 are fulfilled, admits *one and hence also an infinite number of mechanical explanations* (Poincaré).

b. An explanation of a class of phenomena in terms of the theory of energy, may also be made to correspond to an hypothetical mechanism for which the fundamental principles of dynamics do not hold good.

In any case it is well to call attention to the fact that a mechanical explanation, in line with the classic dynamics, can be regarded

as an explanation in terms of the theory of energy, *only under the condition* that the forces contain a potential, that is, that the principle of kinetic energy is valid (Chap. V, § 28).

The theory of energy is then a more general form of mechanics only in case we regard forces without a potential as excluded. This latter case, in which the phenomena do not obey the principle of the conservation of energy, may be regarded as a device used to isolate a restricted form of mechanics composed of incomplete systems, and to replace in the latter whatever is due to forms of energy that are not, properly speaking, mechanical.

§ 17. MATTER AND ENERGY.

The theory of energy, like the classic mechanics, meets with a difficulty in explaining irreversible phenomena, and if it attempts to eliminate the considerations of mean and of limit is obliged to admit *qualitatively diverse forms of energy*, and hence to accept new postulates about the transformations of these forms. Certain new developments in the field of physico-chemistry,[1] developments utilitarian in their tendency, proceed in exactly this manner.

It is not our plan to examine these developments.

Instead of this we will here compare from various points of view the fundamental conceptions of the theory of energy and of the classic mechanics.

We have already shown (Chap. V, § 20) that a critical study of the conditions determining motion tend to distinguish:

1. the *internal* properties of the moving body, to which we refer the representations that lead to the concept of mass;

2. the *external* relations, that is to say the field of *forces* within which the body moves.

The expressions "internal and external" have here a relative meaning. The data furnished by experiments *upon the body* are taken to be internal elements, even in case *other* bodies (instruments etc.) come into play, but such as remain independent of the motion we are trying to determine, and which may be used upon the body, at will, for instance in a state of rest. On the other hand those conditions are regarded as data external to the moving body, in case we can prove their action by bringing into the field any other body whatever (equal in mass). And it is by abstraction that these latter data, which are to a certain degree independent of the moving body, are considered as external to it.

[1] Cf. Duhem, *loc. cit.*

Now we have already had occasion to note that the schematic distinction referred to corresponds but imperfectly with reality, in case we are regarding the known facts in their far wider extension. Indeed on the one hand the comparison of the masses of chemically irreducible bodies leads us to experiments on motion into which forces enter, at least implicitly. On the other hand the Newtonian fields of forces already show us that force, which they try to take for an external datum, is dependent upon the mass upon which it acts. And electric phenomena etc. further indicate a dependence of the forces themselves upon the physical state of the moving body, that is upon its properties which, at least in our first intuition, appear to be internal.

The development of such ideas naturally leads us to regard as fictitious the ideal distinction between the internal and external relations or characteristics of a body, *when taken in a general sense.*

This critique applies to the very idea which we form of *matter,* to which the preceding distinction also belongs.

It should then be understood that to attribute certain properties *to matter* means to recognize the association of certain phenomenal data which are made to correspond abstractly to an object, the *material body,* above all because we connect with that body a certain *localization* and hence the *fact that we can arrange* the phenomena themselves. In this connection *solid* bodies are of special importance, since they also serve as recipients for *liquids* and *gases.*

Now the subjection of matter to disposal corresponds to a series of superposed *constants* that are connected with a group of physico-chemical transformations. *Mass* is one of these constants, connected with a group of all the transformations referred to. Certain fields of forces which we connect with a body, for instance Newton's various forms of attraction, still have a relatively constant sense. And these invariables are expressed by saying that for an *isolated* body the mass is constant etc., where the isolation is a fictitious hypothesis which gives us an abstraction in a simplified form.

Side by side with these constants, and even more general than they are, we find the *energy,* that is to say the sum of the different forms of energy, of an *isolated system.*

The significance of energy itself can be defined, by virtue of its various possible transformations, by reducing it through its equivalence to a determinate form, as for example to *work,* which then corresponds directly to a determinate group of tactile muscular sen-

sations, and can be estimated more precisely by the use of suitable instruments.

Now the school which has found in Ostwald its maker of theories starts with the fundamental view that energy may be regarded as an object in the same way as matter, and that the latter also must be regarded as a *particular form of energy.*

This thesis has raised a great outcry. People have talked about the *immateriality of matter* and the *materialization of energy,* as if under these words a fact of capital importance lay concealed, and also they have appealed to *common sense.* A prudent safeguard for whoever wants to spare himself the critical study of scientific expressions!

The truth is that the expressions reported above do not cloak any doubt as to facts, and that the view that things are energy does not in any way seek to *change reality,* but only to modify our *mental images* of reality, or better, the images that we use in the abstract study of physical relations, because our intuitions as to matter are certainly not driven away by the concrete interpretation of the theory.

The critique of the concept of matter upon which the theory of energy is based, and the conception of energy as an "object," cannot give rise to any well justified objection. But there is room for discussion merely of the value of these ideas in comparison with those suggested by the notion of matter, and especially the conception of *atoms.*

The believers in the theory of energy regard all the partial constants connected with matter, and especially the constancy of mass, as absorbed in the concept of energy. On the other hand they reject every notion of the atomic sort.

These views do not seem to be sufficiently justified, and it is noteworthy that while the atomic theory holds an important office in chemistry, analogous ideas are fruitfully used in the new studies of electro-magnetism.

§ 18. THE LOCALIZATION AND MOTION OF ENERGY.

Furthermore there are two ways of representing phenomena according to the theory of energy.

The first, which serves for partially utilitarian purposes (especially in the recent physico-chemistry), tends to abstract from those visual elements which are connected with *local relations.* In fine,

it carries out the type of the Newtonian mechanical model and brings it to a higher degree of abstraction.

But as opposed to the latter conception there arises a different development of the Cartesian sort, which may be said to be represented by the theory of the *localization and motion of energy.*

Here energy is, so to speak, materialized, since it is compared to a fluid whose density varies in different regions of space, and the explanation of the phenomena is reduced to a description of the motion of this fluid.

These ideas have their source in the theory of electricity (cf. § 21). Maxwell first localized electric energy in the electroscope, and Poynting made clear the simplicity with which the latter moves. Unless there is a deformation which alters the volumes, the flow is normal in the plane of the electric and magnetic forces, and proportional to the area of their parallelogram.

More recently Volterra has proposed a way of localizing the Newtonian energy and elastic energy, and has described their motion—a true optical model of the *transfer of energy.*

We cannot at present judge of the positive value of these speculatively beautiful results.

Still they give us a more synthetic intuition of the distinctions between the internal and the external properties of matter. And they have this kind of interest in relation to our own thoughts: they show how the dualism that exists between the Cartesian and the Newtonian tendencies, which we have connected with the sense-content of our mental images, is still further followed out in the development of the concepts of the theory of energy.

§ 19. THE ELASTIC EXPLANATION OF OPTICAL AND ELECTROMAGNETIC PHENOMENA.

Side by side with the developments of mechanics, in the general theories of elasticity and heat, we should number those concerning light, electricity and magnetism. We will now briefly discuss these.

Physical phenomena of this order are specially connected by two means with the general concept of elasticity:

1. The representation of light as the vibration of an elastic medium (Fresnel's theory of undulations).

2. Maxwell's theory which makes of the insulator interposed between electrified bodies, an elastic medium whose deformations

directly produce certain pressures or tensions, which we perceive as electro-static actions.

The union of these two views led Maxwell to found that general *electro-magnetic theory* to which the subject of optics is subordinated.

§ 20. OPTICS.

Without entering into a minute historical study, we can easily sketch the origin of those ideas which have led to a mechanical theory of light. It will suffice to bear in mind:

1. The analogy that certain optical and acoustic phenomena have to each other and also to certain elementary mechanical phenomena (rectilinear propagation, laws of reflection, etc.).

2. The possibility of obtaining a concrete mechanical explanation of sound, by the aid of the directly verifiable vibrations of sonorous bodies.

3. The fundamental fact that light has a finite velocity of propagation, although a very great one.

Notwithstanding the analogies thus pointed out, Newton, to whom we owe the mechanical theory of sound (a theory later corrected in one point only by Laplace), Newton, I say, founded the first optical theory upon an essentially different hypothesis, that is upon the *emission* of a substance from luminous bodies. It is not difficult to explain the reason of the different way that was chosen.

Because the phenomena of light are not capable of being explained by the motion of matter which falls under the observation of our senses, the hypothesis of emission comes to fill the void with an assumption which has, so to speak, a minimal degree of abstraction, and which furthermore rests upon certain spontaneous associations, traces of which are to be found in our ordinary speech. Let us add that the idea of still more completely likening light to sound, by means of a theory of wave-motions, ought to have met, as in fact it did meet at the outset, with certain serious difficulties.

However, the theory of emission reached in the hands of Newton's followers a high degree of development and, thanks to a certain number of supplementary hypotheses, led Laplace to give an account of the elementary properties of reflection and refraction.

But the progress of the experimental study of this latter phenomenon, and especially the measure of the velocity of light in differently refracting media, were seen to be irreconcilable with the

totality of the hypotheses regarding emission and so, over against the hypothesis of Newton, the rival theory became established,—the theory originated by Huyghens, developed by Young and Fresnel. This theory, enhancing the analogy between sound and light, tends to represent the phenomenon of light as the vibration of an elastic medium, *ether*, which fills all space.

And this theory showed its fruitfulness more and more, succeeding (in the hands of Fresnel himself) in explaining the phenomena of diffraction, foreseen by Poisson.

Now the fundamental problem that Fresnel was to solve consisted of establishing an adequate conception of the elasticity which was hypothetically attributed to ether.

In the first place the simplest hypothesis which consists of comparing ether to an extremely rarefied gaseous medium, is seen to be irreconcilable with the facts. Indeed the phenomena of polarization and of interference, together with the results of the measurements of intensity in the study of reflection and refraction, necessarily lead us to assume that the *vibrations of light are transverse.*

However, ether appears to be endowed with an elasticity that is in a certain sense opposite to that of gases, because in the latter only longitudinal and not transverse waves are transmitted, and the flow of the molecules does not meet with any elastic resistance.

Now how shall we represent such a medium?

If we do not wish to deviate from the habitual conditions of a mechanical model, we must either represent this medium as being like a solid, endowed with *incompressibility,* or on the contrary we must attribute to it *infinite compressibility.*

This second hypothesis was generally adopted by Fresnel, while F. Neumann and MacCullagh developed another theory in which they systematically adopted the former hypothesis.

The two views lead us to suppose that for polarized light the vibration of a particle of ether is respectively perpendicular or parallel to the plane of polarization. But it seems difficult to decide between the two cases by experiment. And Poincaré's analysis tends to show that the difference between them constitutes an indifferent hypothesis, at least in the special optical field of bodies at rest. However, a recent photographic experiment by Wiener is interpreted by him as a confirmation of Fresnel's opinions.

However much is gained in the facility of representation, it plainly contains much that is subjective. Fresnel's ether is a completely new elastic medium, which we can succeed in imagining

only if we think of a gas in which the function of elasticity is substituted for that of viscosity. The hypothesis of solid ether places more difficulties in the way of a representation of the relations between ether itself and matter, and obliges us to postulate a particular force (called Kirchhoff's force) in order to explain the different elasticity of the ether upon the two faces of the surface which separates diversely refracting media.

We shall later have occasion to see how the elasticity of ether is explained from a new point of view by means of stationary movements (§ 24). But in relation to the new conception also, the reciprocity of explanations to which we have referred is represented in the theory of light.

§ 21. ELECTRO-STATICS.

The attempts at a mechanical explanation of electric and electromagnetic phenomena, may be connected with two fundamental views of Clark Maxwell, which ought to be superposed and joined together in one synthetic theory. But there always remains a difficulty of agreement between these views, which their author has never completely succeeded in conquering. While investigating and criticising the facts with powerful logic and with an admirable intuition for analogies, in the separate domains of experience, Maxwell preferred to retain various partial constructions, with a confidence that the apparent contradictions ought to disappear in a more extended view of physical relations, rather than to be arbitrarily eliminated for the sake of systematic consistency.

A first kind of analogy, which in Maxwell's work forms a doctrine by itself, is the *elastic theory of electro-static action.*

Just as optics suggests the idea of an elastic medium, through which the light-waves are transmitted, so the attraction and repulsion of electrified bodies at a distance leads Maxwell to postulate a *sui generis* elastic medium interposed between the conductors. The pressures and tensions of the medium are simply transformed into electric action. And this representative hypothesis has its foundation in Faraday's experiments, from which follows the important part played by the insulators in electric phenomena.

As we see, Maxwell's attempt tends to reduce the forces acting between distant bodies to forces exerted by contiguity. In any case there are serious objections to the system of images in which this guiding idea has been expressed.

Beltrami observed that the tensions produced in Maxwell's

medium do not satisfy the differential conditions that in general hold good for *elastic media* in case we admit as a characteristic property of the latter, that the state of tension produced by an infinitesimal deformation depends, for every element, upon such a deformation, and not upon the succession of states through which the body has previously passed.

These objections do not destroy the possibility of an elastic explanation of electro-static action, but they show that such an explanation cannot be given in the simple form proposed by Maxwell, in which the tensions depend upon the local state of the forces at work. We can indeed, in another way, make a well determined deformation of an elastic medium brought into equilibrium by the action of the forces we have mentioned, correspond to a field of electro-static forces, just as in general to any field of forces. In this manner Somigliana has solved the problem of pointing out the deformation and the corresponding state of tension that belong to an elastic medium that is capable of explaining electro-static action.

But the tensions here depend upon the whole given field of forces.

§ 22. ELECTRO-MAGNETISM.

In the treatment of electro-magnetic phenomena Maxwell was also guided by the idea of explaining forces working at a distance by means of actions taking place through contiguity. Without restricting ourselves too closely, let us try to interpret the thought which governs this method of treatment.

We will start from some analogies with the mechanical theory of heat.

As in the latter theory we are led by the relations between heat and work to the hypothesis that heat corresponds to an internal motion of the particles of bodies, just so the facts of electro-magnetic induction, with which the production of mechanical work is connected, suggest in an analogous way the idea that electro-magnetic energy should be regarded as a form of movement.

Maxwell insists especially upon the analogy between the phenomena of auto-induction of the currents and the forces of inertia, so that he may draw thence the view that "something that moves" corresponds to the current. But, because the phenomena of which we have spoken depend upon the form of the circuit and upon the surrounding medium, he rejects the hypothesis that the thing that

moves is the electricity in the conducting wire. An intuition similar to the *synthetic* spirit of Faraday suggests to him rather to think of the movement of a fluid which fills the surrounding insulator.

Now the question is how to explain by such an hypothesis the various phenomena of electro-magnetic induction. This is precisely the purpose of the theory which Maxwell develops in the fourth part of his Treatise on electricity and magnetism.

And the explanation is found in the direction of the mechanical theory of energy (§ 16), the ideas of which here find for the first time an important concrete application.

Let us expound the guiding principles of Maxwell's theory, referring for the sake of simplicity to the case of the electro-magnetism of *bodies at rest* in a *homogeneous medium.* It is impossible to determine the hidden mechanism which corresponds to the visible facts, but we can in any case express by means of Hamilton's principle the relations which exist between two given forms of energy in the field of experience. That is to say, we can express the relations between the *electric energy* which (independently of the more particular hypotheses of the preceding paragraph) may be regarded as *potential energy,* and the *electro-magnetic energy* which is to be regarded as *kinetic energy* or *vis viva.* The electro-static theory gives us the expression of the former kind of energy as a function of the electric discharges, of the distances, etc. The way in which the second kind of energy depends upon the intensity of the electric currents and upon the geometrical relations of the circuits has been critically examined by Maxwell.

Now these *relations of energy* lately described *well represent the facts of electro-magnetic induction.*

From these relations we next pass naturally to the *equations of an electro-magnetic field,* which express the fact that the future state of the field is determined by its actual state, and in which figure diverse connected elements which enter into the integral combinations representing the two energies above referred to.

The positive content of the equations just mentioned will later be made clear, according to Hertz and Levi-Civita.

An adequate mental image of the hidden mechanism of electro-magnetic phenomena does not arise from Maxwell's developments, but only the scheme of a medium which may be roughly represented as a *cellular system containing a fluid.*

The motion of the fluid corresponds to the electro-magnetic energy, and the elastic reactions, that is to say, the pressures and

tensions determined by this motion in the walls of the cell, give rise to electro-static energy.

We have already shown that the importance of this scheme consists in the representation of the connections between the various electro-magnetic phenomena, and especially between the phenomena of induction.

It thus becomes possible to pursue the imaginative view of these phenomena beyond the field of experience. And precisely so Maxwell was led to his most important discovery.

He observed that extremely rapid periodic electro-magnetic oscillations ought to produce phenomena similar to those of light. The light-waves can then be regarded as a particular case of these oscillations, corresponding to an extremely small length of wave.

These theoretic opinions are to a limited extent confirmed in Maxwell's work. The most striking confirmation is the evidence that the velocity of light is perceptibly equal to the relation of the two units of electric quantity in the electro-static and the electro-magnetic systems of measurement. The comparison of the optical and the electrical properties of bodies gives less satisfactory results, if we require rather precise quantitative agreements.

Twenty years later the electric oscillations were experimentally realized by Hertz, and hence their velocity of propagation measured, the velocity of light was found to conform to the theoretic prediction.

From that day great numbers of new analogies between optical and electro-magnetic phenomena have been found. For example the various experiments upon reflection, refraction etc. have been repeated in the case of the electro-magnetic waves. The work of August Righi[1] has made especial progress in this direction.

We may therefore say that the general concept of the electro-magnetic theory of light now ranks as a sure gain for science.

§ 23. THE POSITIVE CONTENT OF MAXWELL'S THEORY.

In Maxwell's work we find overlapping various partial constructions which on the one hand tend towards a mechanical representation of optic-electro-magnetic phenomena, and on the other hand result in a synthesis of various phenomenal data, expressed by the equations of an electro-magnetic field.

Now elements of various classes figure in these equations, that

[1] Cf. *L'Ottica delle oscillazioni elettriche,* Bologna, Zanichelli, 1897.

is, data belonging to the mechanical model as well as data defined by possible experiments. Hence a critical work is needed which shall aim at recognizing the positive content of Maxwell's theory.

Just such a critique led Heaviside and Hertz to transform and simplify Maxwell's equations.

Hertz especially, by abstracting from the application of Maxwell's equations to anisotropic crystal media deserves the credit of having clearly explained the significance of those equations in two classic memoirs, respectively devoted to the electro-magnetic theory of *bodies at rest* and of *bodies in motion.*[1]

We will here refer to the former case.

Suppose that in a homogeneous medium or in a vacuum, we have given several motionless bodies which are the seat of electric or of electro-magnetic phenomena. We thus have defined an *electro-magnetic field,* that is, for every point we have defined, in functions of time, two vectors: the *electric force* and the *magnetic force.* The electric force is the force which would be exerted at every point of the field upon a unitary electric mass which should be brought there. It is defined independently of the distinction between forces of *electro-static* and *electro-magnetic origin* whose resultant it expresses. Magnetic force is defined in a way that is perfectly analogous to electric force.

The definitions given have a positive meaning in relation to possible experiments, apart from the practical difficulty of measuring electric force in electro-static units.

The knowledge of the field of electric and magnetic forces then completely determines the course of the phenomena and especially of the electro-dynamic phenomena, independently of the knowledge of the bodies electrified, of the currents and of the magnets that have served to define the field itself. This is on the basis of a *fundamental hypothesis* that is generally received as an extension of Ohm's law, verified in the case of permanent phenomena: The electric current in a conductor is vectorially proportional at every point to the electric force acting upon it.

The field of electric forces and that of magnetic forces are closely dependent upon each other. Without further ado Hertz assumes as an hypothesis those equations that express this dependence, and which, referring to an isotropic, homogeneous and restraining medium, may be expressed as follows:

[1] *Wiedemann's Annalen,* t. 40, 41 (1890) and *Nuovo Cimento,* t. 28.

1. The variation of the magnetic force in relation to time is proportional to the *curl*[1] of the electric force;

2. The variation of the electric force is reciprocally proportional to the curl of the magnetic force.

If the field is a conductor this second law is modified in the sense that to the variation of the force we have added that which is due to the current, which is (as we observed) proportional to the force itself.

The equations mentioned, which Heaviside also obtained by transforming and simplifying those of Maxwell, determine the knowledge of the future electro-magnetic field, if its actual state is given. They tell us in fact how any local electro-magnetic perturbation is propagated in space and time.

It is necessary to add to the equations of the electro-magnetic field, only the equations of condition which express the *conservation of electricity* and of *magnetism,* the former of which serves to differentiate ether (vacuum) from matter.

We are then able to interpret these equations and, as Hertz shows, deduce from them known physical laws. In the first place those relating to a permanent régime, as for example Ohm's law of closed circuits, Kirchhoff's principles for conductors in derivation, the rule of Ampère and the law of Biot and Savart. Then the laws of induction which experiment teaches us in the case of closed circuits etc.

The verification of the consequences drawn from the equations of the electro-magnetic field, justifies, according to Hertz, the assumption of these equations themselves as the fundamental hypotheses of the theory.

But in order that the verification may really serve to demonstrate the hypothesis, it is needful to show that the latter may in its turn be deduced from certain of the physical laws that underlie experience.

Just this has been done by T. Levi-Civita[2] who by taking up Helmholtz's theory, and correcting it by means of the hypothesis of a time of propagation for actions taking place at a distance, showed that this leads to Hertz's equations.

We will try to explain this important result more precisely.

Let us assume that:

[1] The curl denotes a geometric operation upon the vectors, which depends upon the *local* variation of the vector in the field.

[2] Cf. *Nuovo Cimento,* 1897.

1. The principle of the *conservation of electricity* is valid, and hence we can represent the variation of an electro-magnetic field as the motion of a fluid that undergoes condensation wherever matter is found;

2. Coulomb's laws of electro-static action, Biot's and Savart's laws of electro-magnetic action, and F. Neumann's[1] laws of electro-dynamic induction are valid when corrected by the hypothesis of a *velocity of propagation* proportional to that of light.

Then we can calculate for every point of the field the electric force from the two components of electro-static origin and of electro-dynamic origin, and the magnetic force given by Biot's and Savart's law. These two forces, for every homogeneous and isotropic medium at rest, are connected by the equations of Hertz.

We can then conclude that: *The developments of Hertz and of Levi-Civita bring to light the positive content of Maxwell's electro-magnetic theory. For homogeneous and isotropic media at rest, this theory is equivalent to the sum of the principle of conservation of electricity and of Coulomb's law, as well as those of Biot and Savart and F. Neumann, corrected by the hypothesis of a time of propagation for actions taking place at a distance.*

§ 24. ELASTICITY CONSIDERED AS MOTION.

While the positive study of Maxwell's theory results in eliminating from the equations of the field all distinction between electric forces of an electro-static origin and forces of electro-magnetic origin, even speculations about the mechanical model of the phenomena tend from another direction to do away with this distinction.

Maxwell had regarded electro-static forces as *true elastic forces,* and electro-magnetic forces as *forces of inertia* or *apparent forces* due to *concealed motions.* A development of the mechanical representation in the Cartesian manner leads us to regard electro-static action also as the effect of concealed motions, and more generally to explain *elasticity as a form of motion.*

This idea occurred for the first time to MacCullagh (1839), who in explaining the optical properties of crystals was led to picture luminous ether not as an elastic medium properly so-called, but as a medium in which certain stationary motions take place

[1] Levi-Civita expressly points out that it is essential to choose F. Neumann's potential law from among the various elementary laws of induction for open circuits which are compatible with experiments relating to closed circuits, and he deduces from that an argument in support of the latter law as compared with others that have been proposed.

which resemble a *rotary elasticity*. Fitzgerald noticed that this model is excellently well adapted to harmonize the representation of optical and electro-magnetic phenomena. But the difficulties of picturing these phenomena have been conquered by W. Thomson with the aid of the concrete model of a *gyrostatic ether*.

The foundation of the construction consists of the property of the permanent axes of rotation which is seen in the gyroscope. With four gyroscopes jointed in lozenge fashion we can compose a system which represents the elasticity of a spring. If we impart to the whole system a rotation, the axis offers a resistance to being turned aside from its own direction. Hence by uniting together an infinity of such elementary systems, we could reproduce the model of an ether fitted to represent the rotation of the plane of poralization of light in a magnetic field (phenomenon observed by Faraday).

A further development by Thomson of his constructive idea led him to think out a *hydro-kinetic model* of electro-dynamic actions, in which the impenetrability and inertia of the matter, in which stationary motions are taking place, produce forces apparently analogous to the Amperian flow of current.

Together with the latter model we should quote that proposed by C. A. Bierknes for the electro-static (or Newtonian) actions, in case these actions are produced as the effect of the vibrating motion of *pulsating spheres* within a liquid.

It happens however with the models to which we have referred that the simulated actions are the inverse of the real actions. But Poincaré has shown how this inversion may be gotten rid of by modifying the interpretation of the models themselves.[1]

Moreover there is another difficulty in Bierknes's model. He must in fact assume that the pulsating spheres have an equal period and an equal phase (or phases differing by π), a thing which seems inadmissible.

This difficulty can be disposed of by supposing a motion of continuous contraction or dilatation, instead of alternative.

This case precisely corresponds to the representation of an electrified particle in case that we construct an extension of Fresnel's optics adapted to electro-magnetic phenomena. This would be a theory in some sort the reciprocal of that recently constructed by Larmor as an extension of MacCullagh's and Neumann's[2] optics.

While the speculations just sketched aim at a concrete represen-

[1] *Electricité et optique*, Paris, 1901, pp. 616 et seq.

[2] Cf. Poincaré, *loc. cit.*

tation of the electric forces as forces of inertia, Hertz was led to imagine that an analogous explanation should hold good in general for all forces; that is to say that the *motions of connected and concealed masses* could be substituted for all forces.

In Hertz's posthumous work on mechanics there is actually to be found an abstract justification of this opinion, and the outline of a treatise on the science of motion from which every actual notion of force was banished.

The fundamental hypothesis is then that there exist invisible masses, connected with visible matter, in such a way that every phenomenon, and especially every motion of visible matter, means in general a motion of the invisible. The law of motion is then reduced to a *generalization of Galileo's and Newton's postulate of inertia.*

Every isolated system moves in such a way that the succession of its positions corresponds to a condition of minimum, which can be expressed by Gauss's *principle of least effort,* analogous to that which the straight line satisfies, among the possible paths of a particle in space, and the great circle among the possible paths of a point on a surface.

Apparent forces are only *reactions* of linkages.

The scheme which Hertz sketched was not carried by him as far as the treatment of determinate problems where we might have seen in what sense and in what way the arbitrary element which remains in the choice of the concealed motions could readily be made to disappear, and what means could be gained from this hypothesis for the explanation of the phenomena.

But the mechanical representations of electro-magnetism above quoted, for example Bierknes's model, may be valuable as an illustration of Hertz's program.

Furthermore we may observe that the various developments of physico-mechanics that we have thus far analyzed tend in general to reduce forces to the elastic type. And therefore in order to realize Hertz's idea it is necessary to build up in complete fashion according to Thomson's opinion, a treatment of elasticity regarded as motion.

§ 25. ELECTRO-MAGNETISM OF BODIES IN MOTION: HERTZ'S THEORY.

The speculative development which tends toward the mechanical explanation of electro-magnetic phenomena leads to a model of the

Cartesian type which is theoretically general and perfect, although as yet not adequately developed in a concrete form. But the speculations as to this hidden mechanism remain too far from the concrete applications of the theory, which as we have seen, has a content independent of its mechanical bases.

In case we are trying to follow out these applications, to perfect and extend the electric model of optical phenomena, the electric forces may be assumed as primary data without investigating their possible reduction.

This is precisely what happens in the equations of Hertz which we considered in § 23, confining ourselves to the electro-magnetism of bodies at rest. So also with the new developments to which the attempt to coordinate the various electro-magnetic phenomena of bodies in motion has led. This is especially noticeable in the theory of Lorentz.

We will give a brief account of this doctrine, leaving on one side the preceding developments of Maxwell. And we begin with Hertz's treatment of the electro-magnetism of bodies in motion, trying to interpret the origin of the guiding idea by which he was inspired.

If we consider an indeformable medium at rest (or so regarded), and determine in it an electro-magnetic field (by means of motionless bodies, electrified, or magnetized, or traversed by currents) we find that at every point the electric and the magnetic force are connected by differential equations, whose content has been analyzed in § 23. Now these equations may be extended in two senses.

In the first place we may assume that they hold good although the *electro-magnetic field* may be *produced by bodies which move within the given medium*. Thus for instance, an electrified body which is moving will give rise at every point to a progressive variation of the electric force, and hence to a perturbation wholly analogous to that generated by a current. This agrees with Rowland's experiments which make the electric current by convection (transportation) similar to the current by conduction (voltaic).

In the second place, if the phenomena are propagated within diverse media which are moving in relation to each other, we may suppose a relative *independence* of these media. That is we may hold that in every one we have an actual electro-magnetic field, defined by those selfsame equations of Hertz taken as *relative to the medium* itself.

Hertz assumes precisely this hypothesis, completing it in connection with the case of *deformable* or elastic material *media.*

He was easily led to this extension by the following considerations.

From the differential equations referring to an inelastic medium, we can infer *integral laws* (one of which Maxwell pointed out) which express the variation of the flow of electric or magnetic force across a closed surface, in terms of the flow of the magnetic or the electric force respectively around the edge. *Hertz assumes as a fundamental hypothesis that these laws are valid for a medium in motion in a sense relative to that medium, that is with respect to materialized circuits that undergo deformation along with the medium.*

Hertz's theory has been presented by the author as a synthesis of hypotheses compatible with a small number of experiments on the transporting of electrified bodies and on the effects of induction of the motion of circuits traversed by currents. And the author himself calls attention to the fact that in these experiments we are always dealing with motions whose velocity is small as compared with the velocity of light.

The need of correction arises from the optical phenomena which appear in the case of bodies in relative motion, and especially in the case of *astronomical aberration.*

Let B be a medium which cannot be polarized, A a source of electro-magnetic or light-waves, and let us postulate, to make our ideas definite, that A is at a great distance from B so as to give rise to slight waves. Thus B may represent our atmosphere and A a star. By hypothesis B is moving with respect to A.

If we take account of the equations of limits in the passage from the medium of A to the medium B, Hertz's theory leads us to assume that the waves sent forth from A are as it were caught up and drawn along by B in its motion, still preserving in B the character of slight waves. From this it follows that the relative velocity of propagation of these waves in B is the very same V that they would have had if B had been at rest in relation to A.

The aberration of light already noted by Bradley contradicts this consequence. In fact that aberration implies that the light coming from the star A continues to move within the atmosphere B with the same velocity V *in relation to* A, and hence (if we denote the velocity of the earth by v) with a velocity $V - v$ in relation to the earth.

In order to explain astronomical aberration, we may attempt

to substitute for Hertz's principle, which expresses a *local relativity* of electro-magnetic phenomena, a *principle of extended relativity* which harmonizes with the synthetic point of view of Faraday and Maxwell.

For the sake of simplicity let us consider an indeformable medium B which is moving in relation to A, and which is the seat of electro-magnetic phenomena.

We may assume that for an observer within B, and transported with the medium, the phenomena appear as if the medium B extended far enough to include A.

To justify such an hypothesis, it suffices to picture the electro-magnetic phenomena as the perturbations of an ether, defined by the *single points* which correspond to the electric charges carried by A.

The procedure of the said perturbations under the influence of the motions of A in relation to B, may be conceived by simply supposing that the ether itself is carried along with A in relation to B.

The same hypothesis may be expressed by regarding the ether as motionless together with A, while B is in motion. If A is a star, B the earth, v the velocity of B in relation to A, V the velocity of the propagation of light in a medium like our atmosphere, we then find that the light coming from the star is propagated in the earth's atmosphere with the velocity $V - v$.

From this comes the phenomenon of aberration, explained in a way that corresponds with Fresnel's opinions expressed in the wave theory.

But the explanation holds good only for the elementary fact observed by Bradley.

The astronomers at Greenwich have measured the aberration with a telescope filled with water. The different velocity of propagation of the light-waves in the water ought here to lead to a different angle of aberration. On the contrary the angle does not change.

Fresnel interpreted this result in the sense that "with regard to water ether is no longer fixed, but is partially carried along."

The *partial transfer of light-waves* in moving matter was later confirmed by Fizeau's experiments, also lately repeated by others. The motion of the water gives rise to a displacement in the fringes produced by two light rays interfering.

From this it follows that the most extended experiments re-

ferring to the optical-electro-magnetic phenomena of bodies in motion cannot be explained even by an *hypothesis of extended relativity* like the one stated above. The phenomena referred to depend upon the relative motion of bodies, upon the velocity of propagation in the medium in which they are verified, and upon *something else,* (pertaining to the experiment), that is, upon the qualitative characteristics of the matter that constitutes the last-mentioned medium.

The question is how to construct a theory that is fitted to take account of the complex conditions of the facts.

§ 26. THE THEORY OF LORENTZ.

The theory of Lorentz[1] (1892) was constructed for this purpose. The fundamental concept consists of attributing the partial carrying along of the electro-magnetic waves by moving matter, to the modification of the field produced by electric charges which, by hypothesis, would be carried along by the moving matter of which we have spoken.

To make these ideas definite let us consider a body A that is regarded as fixed in relation to the ether, and a body B which is moving with regard to A (and to the ether itself). Within B there are superposed two series of perturbations, relative to A and to B, these latter progress with the motion of B, and the *total result is equivalent to the hypothesis of a partial carrying along of the ether within* B.

The preceding case may be generalized by considering a greater number of bodies A, B, C.... in relative motion. In this case there is no longer reason to hold that the one or the other of these bodies, by which electro-magnetic waves are sent out, is fixed with respect to the ether.

To overcome the difficulty Lorentz postulates an absolute system of reference, that is to say an ether independent of matter, whose parts do not vary with respect to each other, and he compares the motion of bodies with this latter ether which is regarded as motionless.

This postulate is actually arbitrary and gives rise to consequences that it will be our duty to examine. We will now try to find out how the first fundamental hypothesis was put into a concrete form by Lorentz, through a system of atomistic images.

In Poisson's old theory there were two *electric fluids,* the posi-

[1] Compare for bibliography Lorentz's article in Band V_2, Heft 1 of the *Encyclopädie der mathematischen Wissenschaften,* Leipsic, Teubner, 1904.

tive and the negative (cf. Chap. II, § 26). Lorentz takes up substantially this conception, but regards the two fluids as made up of materialized particles (electrons) the carrying or motion of which constitutes the currents.

He then adopts Ampère's hypothesis concerning magnetism, namely, that it is the manifestation of currents in the particles of the magnet.

We thus have a unified view of the various electro-magnetic phenomena.

It is especially interesting to see how a skilful comparison of diverse facts has led to the reduction of the three kinds of electric current to a single type. That is:

1. The *voltaic currents* in the conductors.

2. *Electrolytic currents,* which pass through liquids or gases which they decompose.

3. *Convection currents,* that is those which consist of the transportation of electrified bodies.

The unifying hypothesis of Lorentz is based upon the following considerations:

1. Rowland's experiments prove the action of the currents of convection upon the galvanometer.

2. Faraday's laws of electrolysis can be explained (as Maxwell and Helmholtz had already noted) by assuming that the electrolytic currents are due to the transfer of small, invariable electric discharges, connected with certain atomic groups.

3. In particular the facts relating to the propagation of electricity in gases lead to the belief that there is an *electric dissociation* or *ionization* of the latter.

4. The ionization of gases by means of cathode rays, emanating from Crookes's tubes, suggests the view that such rays are discharges of electrons.

5. The permeability of metals by the cathode rays leads to the belief that the electrons can move freely in conductors, since the hypothesis demands that voltaic currents consist of the movements of electrons.

Now in what way do the opinions of Lorentz give us a system of determinate hypotheses, that can be expressed by the equations of an electro-magnetic field?

Without further ado the equations of Hertz are assumed for free ether; the same thing takes place in the case of ether that is contained in matter where there are no electrified particles to be

found, except for the necessity of taking account of the forces which the electrons exercise upon the matter itself. The whole difficulty lies in understanding how the equations to which we have referred should be modified for the field within an electron, to which finite dimensions are attributed. We will not seek to clear up this point, and we will limit ourselves to observing that certain hypotheses are here introduced that are only partially justified *a priori*, and that may later be indirectly confirmed by the consequences deduced.

When the elementary laws of the phenomena have been once indicated, so to speak, we have only to superpose them in order to deduce in general the equations representing, according to Lorentz, the more general electro-magnetic field. Therefore it is necessary, in case of every particle of the *media*, to take account of the effects produced by the motion of the electrons.

Hence we have a *statistical kinetic theory of the motion of electrons*, which calls to mind the kinetic theory of gases.

And the important point is this: the theory not only takes account of the facts in view of which it was constructed, but it affords a good explanation of Faraday's experiments upon the rotation of the plane of polarization of light in a magnetic field, and leads to the prediction of new relations between light and magnetism, especially the decomposition of the rays of the spectrum in a magnetic field, verified by Zeeman.

It is true that new experiments on this phenomenon have led to a partial modification of the previsions, and to some complication of Lorentz's early views, but the range of experimental discovery which the theory reduces to rational order constitutes a genuine success.

And the success is all the more striking in the case under discussion, because it satisfies a need of electro-magnetic optics, the need of discovering new and positive relations between light and electro-magnetism.

§ 27. A CRITIQUE: THE PRINCIPLE OF ACTION AND REACTION.

The interest aroused by the theory of Lorentz, and its success in explaining known facts and in predicting new ones, have called for the critical study of this theory by the most renowned scientists and have given an impulse to more extended attempts at verification.

From the point of view of the classic dynamics, Lorentz's theory has one serious defect: *It does not satisfy Newton's principle of action and reaction.* At least the principle is not satisfied if we

attempt to understand it in the only way that would have any positive signification, that is as *relating to matter.*

It is easy to observe that this consequence results not so much from the particular views of Lorentz, as from the function which, according to Maxwell's ideas, is attributed to ether as the propagator of actions apparently at a distance.

A body receiving light is subjected to pressure, as already foreseen by Maxwell and Bartoli, and experimentally verified by Lebedef; and although the experiments concerned are very delicate, the result seems the more credible inasmuch as it nearly approaches the facts as to the motion of comets, which had already led Fay to conceive a similar hypothesis.

Now this pressure of Maxwell and Bartoli does not agree with Newton's principle of action and reaction, because the moment when the light reaches a body does not coincide with the moment when it leaves its source.

But this is not all. A thorough analysis made by Poincaré has shown that the violation of Newton's principle is necessarily connected with every electro-magnetic theory which attempts to take account of the *partial drawing along of the light-waves.*

We shall be able to understand this necessity if we take up for a moment the hypotheses of Hertz and examine them.

Between two electrified bodies A and B relatively at rest, certain electro-static forces are exerted, in agreement with Newton's principle. But if A and B are moving, and this action is propagated with a finite velocity, the force which acts upon every point of A is no longer in a direct line towards the points of B from which it emanated. Therefore there supervenes as a corrective term in Hertz's equations, the Amperian action between the *elements of the currents* which correspond to the motion of A and B. And therefore Newton's principle is satisfied by these complex actions.

But now let us suppose that the propagation of the force emanating from A to B is modified by the motion of a medium which accompanies B (this is in substance the hypothesis which corresponds to that drawing along of which we have spoken); this tends to modify the direction of the complicating force acting upon B, that is, it tends to invalidate that accidental compensation which takes place in Hertz's theory. Newton's principle of action and reaction no longer exists!

And since this conclusion results, not merely from the special

hypotheses of Lorentz, but from Fizeau's experiments, we are forced to accept the downfall of the principle itself as a fact.

We will discuss the consequences of this later.

§ 28. THE PRINCIPLE OF RELATIVITY.

Next comes another still more serious fact, which is only apparently connected with the previous facts.

Lorentz's theory permits us to foresee the possibility of ascertaining the *motion of matter in relation to the ether,* that is to say, a true *absolute motion* which does not depend upon relations amongst bodies!

There is some discussion as to the possibility of a verifying experiment. It was found at once that in experiments on this earth the phenomena of light cannot enable us to ascertain the motion of the earth in such a degree of approximation that the square of the astronomical aberration may be neglected. But as we shall relate, more delicate experiments have been imagined by means of which we ought to gain an appreciable result: the issue has been negative.

To tell the truth, this is not at all surprising; but it is well to take account of the motives which lead to such a strange prevision in Lorentz's theory. One will easily perceive that the successes of a theory do not really, as one might think, form any presumption *a priori* in favor of an hypothesis that has been invalidated by experiment.

Lorentz gained his conception of the electrons from a skilful synthesis of various facts. The value of his hypotheses is derived precisely from the preliminary observations contained in them. But in referring the motion of the electrons to a fixed ether, independent of bodies, the illustrious physicist followed an arbitrary criterion, however convenient for the purposes of mathematical treatment. Now it is precisely this arbitrary hypothesis, this species of absolute placed at the base of the theory, which is again found in the further deductions; deductions which from this point of view do not become more credible for having been joined with hypotheses having such a notable origin.

But let us examine the consequences to which we are led by the developments of the theory.

A system of matter S is moving in relation to the ether with a translation which, for example, is uniform.

Two electrified material particles A and B, connected with S, no longer exert a simple electro-static action between them, but generate

two currents the Amperian action of which diminishes the former first. To tell the truth, this correction would have to be considered in a positive sense as non-existent if it did not depend upon the position of A and B in relation to the direction of the translation, because in such a case it could not be measured by an observer carried along with S, but only by one who could make comparisons with what was taking place in the immovable ether. But the Amperian action referred to depends upon the direction of the straight line AB, and thus becomes practically verifiable by experiments within S.

An actual optical experiment, completed by Michelson in 1881 and repeated by the same investigator together with Morley in 1887, corresponds precisely with the problem set us in the foregoing considerations.

The problem is schematically dealt with as follows:

In S three points A, B, C, are given, the vertices of an isosceles triangle, rectangular at A; AB is the direction of the movement of translation of S, AC is perpendicular to the latter motion; then the velocity of the propagation of light from A to B, and from A to C should be different.

The experiment tried upon the earth, taken as a system S, does not enable us to ascertain any difference. Very good—the calculation of this difference shows that it is of the degree of the square of the aberration and therefore it ought to be appreciable!

In this way Michelson's experiment afforded a confirmation of the *principle of relativity*: *By means of experiments, optical, electrical, magnetic, within a moving system, the motion of translation of the latter with respect to ether cannot be determined.*

To explain this negative result Lorentz himself, and also Fitzgerald, have conceived the hypothesis that all the lengths of moving bodies undergo a slight shortening in the direction of the translation; hence the distances AB, AC which in the experiment *appear* equal, in *reality* (that is in relation to the ether) are to be considered as different.

To make this shortening plausible (which for the earth is of the degree of the square of the aberration) and to obtain the complete explanation of the principle of relativity (at least in the case of uniform translation) he had recourse to other hypotheses, that is that the molecular forces upon which the dimensions of bodies definitely depend are of electro-magnetic origin, that matter itself is an assemblage of electrons (§ 31) and that the latter instead

of preserving an invariable spherical form (Abraham) undergo a contraction as if the ether pressed upon them in their motion, and so they are reduced to flattened ellipsoids, either preserving the same volume (Bucherer, Langevin) or retaining two equal axes (Lorentz).[1]

This last hypothesis is really the only one which, combined with the preceding ones, takes account of the principle of relativity, and according to Poincaré[2] is justified by assuming that the ether exerts upon the deformable and compressible electrons a constant pressure, whose work is proportional to the variation of their volume.

Now the hypotheses indicated above cause a radical transformation of the classic dynamics, which is being replaced by a new electric dynamics, of which we shall speak later. We here limit ourselves to summing up the results to which we have been led in the question under discussion.

The principle of relativity in the case of electro-magnetic phenomena can be explained according to the theory of Lorentz by means of the following hypotheses and considerations:

1. Bodies are made up of *electrons* which *contract* during a motion of translation, becoming reduced to flattened ellipsoids, through the effect of a *pressure of the ether,* the work of which is proportional to the variation of volume of the electrons.

2. All *forces* are of *electro-magnetic origin.*

3. The *measurements* which are made within a moving system are *relative* to the dimensions of the instruments, to the forces taken as a standard of comparison, and to the *local time.*

Let us imagine the *true time* t to be defined in relation to a fixed observer, bound to the ether. An observer carried along with the motion of matter, who communicates with the former by means of signals, optical, electrical or otherwise, adopts a measure of time τ in which

$$\tau = at + b$$

(cf. Chap. V, § 8) ; a and b are *local constants,* the former depending upon the velocity of the translation (supposed to be uniform) and the latter also upon the distance of the moving observer from the fixed observer.

The theory requires that τ shall be estimated in accordance with the positive indications of the clocks in S, while the agree-

[1] Cf. *Proceedings of the Academy of Amsterdam,* May, 1904.

[2] *Comptes rendus de l'Acad. des Sciences.* Paris, June, 1905.

ment of the different clocks is established in S as if the time employed by the light in passing from a point A to a point B were independent of the direction of the segment AB. Such independence no longer holds good in relation to the true time, according to the hypotheses of Lorentz.

§ 29. ETHER AND MATTER.

The explanation of the principle of relativity in the case of electro-magnetic phenomena, has been put by Poincaré (*loc. cit.*) into a suggestive mathematical form, which we may express as follows:

There is placed at the basis of geometry and especially of the definition of the congruence of the figures, the group of transformations (*geometrical motions*) which correspond to the hypothetical movements of solidified ether. Then the transfer of solid bodies within a moving material system S, the measures of the time required for the propagation of light etc., define equally within S a same *apparent* congruence of the figures, affected by the motion of S in relation to the ether; and this expresses an invariant relation, not now in relation to the group of geometrical motions, but rather in relation to a *new group of transformations* (the group of *apparent* motions, transformed from the *real* geometrical motions by means of a transformation of Lorentz).

In this statement a transcendental motion of congruence is seen in contrast with the experimentally defined notion; the former being taken as a *metaphysical reality,* the latter as a *physical appearance*!

A similar consequence is connected with the representation of an *ether independent of matter,* which is used as the basis of the theory.

But critical study tends to free itself from this sort of absolute. It relegates the hypothesis of the fixity of the ether to the realm of those hypotheses which cannot fall even indirectly under the control of possible experiments, and in fact deprives it of all significance, and recognizes in it only a mere artifice.

This artifice may seem useful for the sake of mathematical treatment, but we cannot deny that it has its dangerous side, since it has been able to justify for some time doubts which had no credible foundation. In any case we shall not greatly regret these doubts which, openly confronted, and especially by the founder of the theory, have given rise to an attempt at the unification of forces, which appears to give an attractive promise for the future.

We will only point out the way which, in our opinion, ought

to eliminate *a priori* the fictitious questions which arise from the artifice we have mentioned.

What we need is to get a more satisfactory notion of *ether*, which ought to be *in relation to matter*.

We should not forget that ether is only an intermediary of the relations between material bodies. For this reason these bodies are actually the *datum*, and it is proper to start from them in order to *construct* a notion of ether.

Now the relations between ether and matter have been investigated by the study of various mechanical models (§ 24), but generally from an opposite point of view. With a certain propriety we may imagine given a fluid with certain properties, a simplified image of real fluids (for instance an infinitely compressible homogeneous fluid, or one analogous to a solid, movable or immovable etc.) and we may consider matter as the place of single particles of this ethereal fluid, whether the material particles are conceived as points where the ether is *destroyed* or *holes* in the ether, or are conceived as *vortices* or as points of *torsion* etc.

Speculations of this sort are instances of the attempt to comprehend *by means of analogics*, the relations of phenomena which are conceived as connected with the *totality* of bodies. The value of these speculations consists precisely in the view that there is a *universal solidarity* of all perceptible things. This view is opposed as a corrective to the very intuitions of matter, that is to the distinctions of internal and external characteristics which we connect with matter.

But if we are to express the solidarity of the world of the senses, and to *correct* the distinctions abstractly acquired with respect to that world,—it will be difficult adequately to succeed in such a task by means of a model chosen *a priori*!

Hence there is no cause for astonishment if even the models invented by talented men give but a poor account of the relations which they are intended to explain. Nor need we be surprised if theories abstracting from all mechanical models are equally useful for the purposes of knowledge.

Let us demand that ether should be defined as something which in every place and at every instant is in the closest connection with all matter near or distant. Perhaps for example we think of it as a collection of particles which emanate from material bodies, or which are concentrated in them; particles whose motions are connected with the phenomena of matter and can be known, at least

in their indirect effects, by means of the physical relations between bodies.

We believe that some unifying representation of this sort will before long appear in the field of the physics of radiation,—a field in which at present emissive radiations (discharges of electrons) and vibratory radiations (which do not differ from a magnetic force) are considered to be radically distinct. This will be in some sort the resuscitation of an optics of emission, radically transformed.

Without pausing over the difficulties of this programme, we confine ourselves to pointing out that the conception of ether which we require would *a priori* eliminate from the electro-magnetics of moving bodies, the fictitious questions which arise from adopting an absolute frame of reference. We should only need to know in case of every medium and every class of phenomena which are developed, what are the ethereal particles to which the phenomena themselves should be attributed. And if in the case of phenomena originating within the medium we have intervening only the particles that emanate from bodies belonging to that medium, the principle of relativity is assured *a priori*.[1]

§ 30. THE DYNAMICS OF THE ELECTRON: RADIATION.

We have shown (§ 28) how the explanation of the principle of relativity in the case of electro-magnetic phenomena has been obtained through the more recent developments of the theory of electrons, from which has sprung a science of *electro-dynamics*.

The point of departure of this new doctrine is the study of the various radiations passing out of a magnetic field (cathode rays, rays of Becquerel etc.) which in order to explain their properties we are led to regard as discharges of electrons.

If we accept this hypothesis (of Becquerel and J. J. Thomson), we have in the radiations referred to an example of electrified bodies which move with enormous velocity, scarcely inferior to that of light. The velocities which appear in the ordinary dynamics are extremely small by comparison.

In order to deal with the motion of the electrons in the cathode rays etc., we should bear in mind that the acceleration or the retarding of this motion implies a current, which modifies the electro-magnetic field. By taking account of this modification it is possible to build up the *dynamics of the electron,* the principles of which

[1] The views more recently developed by Walter seem in a certain way to approach this class of essays. Cf. *Rivista di Scienza,* Vol. VI, 1908.

will find exactly in the study of the rays we have mentioned an application and also a confirmation (Abraham, Schwarzschild, Kaufmann etc.).

The vector equation of the motion of an electron may be stated in a form analogous to Newton's equation of the motion of a material particle. Let us write out the latter, letting m stand for the mass of the particle, a_g the acceleration and f_g the force, in the direction g. We thus have:

$$ma_g = f_g.$$

Now instead of m we ought to substitute an expression of the type $m + m_g$, and thus we get:

$$(m + m_g)a_g = f_g.$$

The quantity m_g which is added to the actual mass m of the electron is not now a constant, but depends upon the geometrical form of the electron itself, upon its electric charge, upon the amount and direction of the velocity with respect to g. This quantity m_g has been called the *electro-magnetic mass.*

Let us apply the equation just given to the case of rays considered as the discharges of negative electrons.

An electric or magnetic force turns aside these rays, and from the deviation obtained, from the calculation of the charge transferred and the heat developed, we can deduce in various ways a measure of the complex mass $m + m_g$. In the case of rays the velocity of which is nearly one-tenth of that of light, it is found that their mass is about a thousand times smaller than that of an atom of hydrogen.

But in the case of cathode rays with great differences of potential between the electrodes, and having velocities which mount to $9/10$ that of light, Kaufmann has found far superior masses.

The various results of experiment may be well represented by supposing that the electro-magnetic mass m_g is far greater than m, and also by assuming $m = 0$. In such an hypothesis the mass of an electron is purely electro-magnetic, that is to say the production of the *forces of inertia,* which intervene to accelerate the motion of an electron or are generated by retarding it, *is explained as an electro-magnetic phenomenon.*

§31. ELECTRICAL DYNAMICS.

The dynamics of the electron leads to an *explanation* of the Newtonian dynamics, which here appears as an approximate theory of motions of a small degree of velocity. (Wien, 1901).

Let us first suppose that a material particle may be regarded as an electron, and let us consider for example the electro-magnetic mass m_1 of the latter in the direction of the motion (longitudinal mass). If we let V be the velocity of light, v the velocity of the electron, e its charge, r its radius (taking the electron to be spherical), we have

$$m_1 = m_0 \left(1 + \frac{6}{5}\frac{v_2}{V^2} + \dots\right)$$

$$m_0 = \frac{2}{3}\frac{e^2}{V^2 r}.$$

By analogy the *transverse mass* of the electron (relative to the direction perpendicular to the former) is developed by means of a series proceeding by the equal powers of v/V, whose first term is always m_0.

Hence we deduce that *if the v is small in comparison with* V, *the electro-magnetic mass of the electron is appreciably reduced to the constant* m_0.

This actually means that, for small velocities, Newton's law holds good approximately as the law of the motion of the electron.

Now from the dynamics of the electron we pass to a new electro-dynamics of bodies, with the aid of certain hypotheses about the constitution of matter and about forces. Hence we are led to extend the electrical explanation of Newton's law of motion to the case of a material particle, which in fact can no longer be regarded as a single electron, but rather appears to be an aggregate of electrons.

An *electrical theory* of matter was developed by Lodge in 1902. It consists of regarding the atom as made up of a system of positive electrons and of negative electrons, the latter moving around the former like satellites.

This determination of the model results from the comparison of the various radiations, especially the differences between the rays of the negative electrons and the rays of the positive electrons (the canal rays, and the α rays of radium).

The electric theory of matter leads to an *electric theory of forces.* The forces within matter (elastic, chemical etc.) are represented as resulting from the actions that take place between the electrons of which the matter is made up. Even gravitation, as we shall set forth, admits of an electrical explanation.

Hence we infer that the laws of the motion of a body, regarded

as a material particle, are the same that govern the motion of an electron. That is, the motion is represented by the vector equation

$$f_g = m_g a_g,$$

which is, *in form*, analogous to Newton's equation, but in our equation the *electro-magnetic mass* m_g is no longer a constant, but depends upon the velocity and the direction of the motion with respect to that g of the force.

Now m_g is the sum of the vectors corresponding to the electrons of which the moving body (material particle) is made up, and hence it depends:

1. Upon the *internal* movements of the electrons in the atoms or molecules.

We cannot say that the velocities of these motions are negligible in comparison with that of light, because the hypothesis happens to make them notably superior to the velocities of ordinary motions. But if the dimensions of the particle of matter are large, in comparison with those of the electron, the total electro-magnetic mass which corresponds to those internal motions results from a computation of averages, and so may be regarded as a constant.

2. m_g also depends upon the *external* or visible motion of the material particles.

But as the velocity here concerned is small in comparison with the velocity of light, this external motion does not perceptibly modify the electro-magnetic mass.

Then m_g may definitely be regarded as a constant, that is to say: *for relatively small velocities the laws of motion of electrodynamics find an approximate expression in Newton's laws of dynamics.*

This conclusion is very interesting. *Instead of explaining electro-magnetic phenomena by means of a mechanical model, it gives us an electrical model of mechanics itself, which implies rather a presumable correction of the latter.*

Such is the result to which we are led by the most advanced attempt to unify forces, by studying in the direction of Newton's ideas. We thus get a *non-Newtonian dynamics* from which the classic laws follow as a limiting case!

§ 32. THE ELECTRICAL EXPLANATION OF GRAVITATION.

Further on we shall study more thoroughly the significance of this inductive development of dynamics. In the meantime let us

see what positive consequences follow from it for the theory of gravitation, in relation to the astronomy of the planets.

By taking up an idea of Massotti's, Lorentz (1900) formed the hypothesis that Newton's attraction between bodies could be explained as resulting from the action taking place between the electrons contained in them. For this it suffices to assume a small difference between the attraction and the repulsion that an electric charge exerts upon two other equivalent charges of the opposite name.

The electric theory of matter supports this hypothesis.

Now as a consequence of this latter, it follows that gravity must be propagated with the velocity of light, a result which seems to contradict the conclusions of Laplace and Lehman Filhés (cf. Chap. V, § 29).

But for these authors the time of the propagation of gravity was the only corrective hypothesis introduced.

On the other hand Poincaré, who takes account of other small corrections, in relation to the hypothesis of the electro-dynamics already indicated, announces[1] that he has succeeded in discovering a certain compensation, by which the divergence from Newton's law instead of being of the degree of the astronomical aberration (as in the calculation of Laplace) reaches the degree of the square of that aberration.

It then seems possible that the propagation of gravity with the velocity of light may be reconciled with astronomical observations.

And this is what further researches ought to tell us.

Especially if from such researches there is obtained a correction of the little deviations from Newton's theory (Chap. V, § 29), the hypotheses of electrical dynamics will thus be better confirmed.

§ 33. CONCLUSIONS: GENERAL NON-NEWTONIAN DYNAMICS.

Let us compare the developments discussed in the preceding sections, and see what conclusions result from them in connection with a more precise verification or correction of mechanics.

The guiding idea of pursuing the study of physics as an extension of dynamics is found to be, up to a certain point, justified *a posteriori* by the success of the previsions deductively established, upon the basis of observations and experiments which in their various domains afford the complementary hypotheses.

However, the success is not complete, and summing all up we must admit that:

[1] *Comptes rendus, loc. cit.*

1. These verified conclusions often have a generality greater than that of the premises, so that they do not serve to furnish any more precise demonstration of the hypotheses of classic mechanics than that which is deduced from direct observations and experiments (§ 16).

2. The mechanical explanation of certain phenomena requires the addition of considerations of limit and of considerations of averages (§ 15).

3. Other phenomena escape from the prevision of the mechanical theory, and cannot be accounted for under that head unless we agree to accept rather artificial and indeterminate hypotheses, such for example as the phenomena of hysteresis (§ 12).

4. Finally certain optical or electro-magnetic phenomena seem to lead to a direct contradiction of the principles of classic mechanics, especially of Newton's principle of action and reaction (§ 27).

From all this the following conclusion results, namely that:

Physics, instead of affording a more precise verification of the classic mechanics, leads rather to a correction of the principles of the latter science, taken a priori as rigid.

And thus it follows that the correction provided by electro-magnetic optics applies to the *generalized principle of inertia,* according to which Newton's principle of action and reaction is deduced from a static symmetry (Chap. V, § 22).

Now the question is to see what type of hypothesis can be substituted for this principle in a general *non-Newtonian dynamics,* while we regard as true the postulates implicitly contained in the fundamental concepts and laws of equilibrium and of incipient motion (cf. Chap. V, § 23). Hence the question is how to work into this scheme the corrections which have already been brought forward (§§ 12, 31) and especially to show how electro-dynamics are actually reduced to a particular determination of the non-Newtonian hypothesis.

Let us try to give an account of the modification that takes place in the classic opinions, in case we disregard the generalized principle of inertia, and for this purpose we will refer to a case that is schematically simple.

Let several bodies A, B, C. . . ., which are motionless in relation to the directions of the stars, define a field of force within a certain region of space S.

This means that in carrying a material particle P to any position whatever in S we must exert a known force to hold it fast, a

force which for the sake of generality we may suppose to depend upon the mass m of P.

Let us imagine that we impart an impetus to P. The motion of P continues through the action of the initial impetus and of the force defined as above for every position in S. The generalized principle of inertia enables us to *sum up* simply these two determining facts, or to sum up those which get defined at every moment: the velocity of P at that moment and the force which belongs to P by virtue of its *actual* position.

Finally the prevision of the future motion of P is deduced by putting together our knowledge of these determining circumstances:

1. The *internal* characteristics of P, that is its mass.

2. Circumstances *external* to the motion of P, that is the field of force defined by A, B, C....

3. A simple *character of relation* between P and A, B, C...., that is the *velocity* of P in relation to those bodies, at the given moment.

These circumstances define the *actual* state of the phenomenon of the *motion of an isolated particle*, and its *future state depends upon its actual state and not upon its preceding states* (cf. § 12).

This hypothesis of *non-inheritance* corresponds to the generalized principle of inertia. It implies in particular that *the field* S is not changed by the motion of P, at least in this sense. The *presence* of P in S modifies the field of forces defined by the bodies A, B, C...., that is to say it changes the result of the experiments which are performed on other particles held at rest in S. But *this modification being conceived as instantaneous*, we do not have to take account of it in studying the motion of P itself, that is to say the *presence of* P *in the field at a previous moment does not modify the force which acts upon* P *in its actual position.*

Now we will drop the generalized principle of inertia, and (keeping the other concepts and principles of mechanics) we will try to find out the general type of a *non-Newtonian hypothesis,* which must be assumed, instead of the principle abandoned, as the determining cause of the motion.

This hypothesis could make the future motion of P depend upon all its preceding states, that is it might take the form of an *hypothesis of inheritance*. But if we want a representation of the facts by means of elementary causes (Chap. III, § 33), we must assume that these states have introduced through contiguity certain

modifications which we are able to recognize in actual form in the field.

In this way, instead of the *hypothesis of inheritance* we have substituted an *hypothesis of the solidarity* of the field:

If an isolated particle P *is moving in a field* S, *in relation to the immovable bodies* A, B, C...., *which are regarded as circumstances determining the motion, its future motion depends upon its mass, upon its position, upon its actual velocity, and upon the forces which at the actual moment could be virtually determined by the exploration of the field. Such forces depend in their turn not only upon* A, B, C.... *but also upon the preceding motion of* P, *as if* P *were not isolated but contained in a medium which moved with it.*

The representation of this fictitious medium which is contradictory to the experimental conditions assumed in the hypothesis of *isolation* (surrounding vacuum) means that it is not possible to distinguish by abstraction the *internal* and *external* conditions that determine the phenomena of motion, and finally it results in expressing an hypothesis of the solidarity of all things that are perceived by the senses.

The developments of the mechanics of energy and of electromagnetic optics familiarize us, in two different ways, with a similar view. But in our opinion we go beyond the bounds of reason if we try to get rid of the distinctions which are connected with that concept of matter which is obtained in the primitive way (cf. §§ 17, 19.) It would seem more reasonable to retain those distinctions as a first approximation which is needed for the development of knowledge, and which must be progressively corrected by successive integrations.

From the preceding critique it appears that if we abandon the generalized principle of inertia, there is opportunity for the construction of a general non-Newtonian science of dynamics in which we can extend the observation of the data that determine motion,

1. either in time, by means of an hypothesis of inheritance;

2. or in space, by means of an *hypothesis of solidarity,* which may be substituted for the former hypothesis.

The permanent alterations of elastic bodies (phenomena of elastic hysteresis) suggested hypothesis 1 to us. The phenomena of electro-magnetic optics suggested hypothesis 2, which is fitted to give an account of the *apparent inherited motion* which we find for example in magnetic hysteresis, because it suffices to postulate a modification of the surrounding ether.

It will now be well to point out more explicitly that:

Electrical dynamics is a particular case of non-Newtonian dynamics, which arises from replacing the generalized principle of inertia by an hypothesis of the solidarity of the field of motion, allowing the *principles of equilibrium and of incipient motion to remain fixed.*

In fact we have observed (like Wien) that the laws of electrodynamics give us approximately those of Newton's dynamics. And this approximation is so much the greater according as the velocity of the moving body is smaller, so that the laws of equilibrium and of incipient motion prove to be exact.

The fact that the hypotheses of electro-dynamics conduce to the approximative validity of Newton's dynamics, does not depend for the rest upon the particular determination of those hypotheses.

In fact if we study in general the hypothesis of the solidarity of the field of motion of a particle P, we see that this leads us to hold that there is a progressive *local variation of the field* (depending upon the motion of P etc.), which is *propagated with a certain velocity* V.

The generalized principle of inertia corresponds to the assumption $V = \infty$. And we can make this assumption without appreciable error, whenever the velocity of P is *sufficiently small in comparison with* V.

The degree of minuteness cannot be fixed *a priori*, because it depends among other things upon the *frame of reference* of the motion.

The principle of inertia is already verified with a fair degree of approximation, for exceedingly small velocities, in terrestrial dynamics. But the approximation is closer, and applies also to greater velocities, in case we refer the motion to a system of directions that are astronomically fixed. This includes in a certain way the opinion of universal solidarity, with which Newton's dynamics was already obliged to reckon.

From the preceding statements we will draw the following conclusions:

The developments of the philosophy of mechanics represent an attempt to *explain* physics, by recognizing in it the verification of the fundamental *hypotheses* of a form of mechanics, assumed to be both *general and rigid.*

The deductions and experiments carried on during two centuries, on the contrary, seem to invalidate one of those hypotheses,

and lead us to think that in place of Newton's mechanics we ought to substitute a non-Newtonian mechanics in which the laws of equilibrium and of incipient motion should be retained. The recent electro-magnetic theories must also determine the complementary hypothesis of the new dynamics. But apart from this determination, *there is ground for believing that the generalized principle of inertia has an approximate validity that is the more precise according as the velocity of the motion is smaller in comparison with the velocity of light.*

And it seems to us interesting to recognize as the surest part of mechanics, that which represents in fine "*the part that is common to all forms of mechanics relative to any frames of reference whatsoever.*"

§ 34. PHYSICAL EXPLANATION: THE VALUE OF MECHANICAL MODELS AND OF EQUATIONS.

From the developments of the preceding paragraphs we have already drawn a first class of conclusions in connection with the verification of dynamics. Let us now draw from those developments some other conclusions that refer to the *value of mechanical models* and to the significance of the explanations of physics.

To do this we ought to make connections with all that was said at the beginning of this chapter.

The hypothesis of an invisible mechanical substratum beneath physical phenomena may be positively interpreted as a *process of association and abstraction* which leads to a representation of the relations of phenomena by means of the *quantitative relations* of certain data, that is by means of the *equations* that determine the phenomena.

For the rest we have abridged two kinds of explanations, in which there are assumed as elementary data of the supposed dynamic phenomenon the "rigid connections" or on the other hand the "forces,"and which tend to become reduced one to another. These two tendencies which are connected with the names of Descartes and Newton, alternate and are interwoven in that scientific progress whose frame we have been trying to reconstruct. They converge so as to form a more intimate association of the sense-data and a closer connection of the images, from all which we gain on the one hand an enlargement of the concept of the connections, and on the other, forces become unified under a type, for instance as elastic or electric forces.

But in the contrast between the Cartesian and the Newtonian tendencies we perceive not only a difference in the choice of the primitive images, optical or tactile-muscular, but also a disposition to estimate differently the *associative genetic moment* and the *actual abstract moment* in the representation by means of relations of quantity.

From this point of view, the two tendencies seem farther apart than ever, because the most radical theses can be equally well maintained, whether "the physical *explanation* consists of the *mechanical model,*" or on the other hand "it consists of determining *equations, independent of any model.*"

This difference in the way of understanding the "physical explanation" is usually connected with the psychological difference which separates *imaginative* minds from *logical* minds. But besides this there is a difference of function between the two types of explanation in relation to two impelling powers of scientific progress, that is to the actually inventive development in which imaginative minds are especially helpful, and to the systematizing of acquired science, a work best undertaken by logical minds.

The correlative of "explanation," that is "comprehension," means for both classes of minds a skill in making *sure previsions*, but these previsions do not take the right direction equally for all in the same field.

Now it is evident that the *synthetic explanation,* which brings with it the maximum of comprehension, will result from a critical coordination of the various types of explanation, which gives not only the aggregate of the various pervisions which can be required, but also an adequate knowledge of the relations between the different fields to which these previsions refer.

Let us illustrate this concept by developing in the form of thesis and antithesis the two views concerning the cognitive and heuristic value of equations and of models.

Thesis. The possibility of furnishing a mechanical model for a group of phenomena A denotes that the measurable data of these phenomena can be determined at any moment, once the arbitrary parameters are fixed.

The cognitive value for knowledge of the model then *consists of the equations* which make this determination possible, equations which result equally, as the *common part,* from the *various possible models.*

An essential difference should here be observed between the two cases:

1. The equations can become practically independent of any model, inasmuch as those data whose variation we are seeking, are themselves connected. In the latter case the model has served only as an intermediary for the establishment of certain *associations among the phenomena,* in which the *positive content* of the theory consists. Hertz's electro-magnetic theory (§ 23) is an example.

2. On the contrary the quantities connected by the equations may refer not directly to the phenomenal data but to the mental images. In the latter case the equations themselves no longer express the positive content of the theory, nor even the part common to *all* the possible models, but only *the common conditions of a series of models.* Let us call to mind the latter case in Fresnel's wave theory of optics. The fundamental equations are here equally well obtained by means of a few general hypotheses independently of a particular determination of the vibrating elastic medium,[1] but the fact remains that in these equations we do not directly perceive positive relations, because their interpretation is connected with the model.

Antithesis. A mechanical model relating to a group of phenomena A, prolongs the sensations by an imaginative view of them, and hence suggests the discovery of new relations:

1. between those same data of A,

2. or between the latter and other phenomenal data belonging to a more extended group.

This being the case the *euristic value* of the mechanical models adapted to A, actually consists of certain suggestive characteristics of the images, of various possibilities of extension, and in general *of the differences of the models* in question.

Let us compare for example the wave theory of optics and the electro-magnetic theory of optics. The elastic model, by analogy with sound, sooner suggests the discovery of Döppler's principle. The electro-magnetic model, which is equivalent to the preceding for media that are non-conductors of electricity, or that are not permeable by magnetism, permits us on the other hand to predict new relations between conducting and non-conducting bodies (Maxwell) or between the phenomena of light and the magnetization of the field (Lorentz-Zeeman).

[1] Cf. for example Voigt, *Kompendium der theoretischen Physik,* Bd. II, pp. 554 et seq.

In conclusion let us state that the two ways of *explaining* physical phenomena, by means of equations or of models, correspond to two different forms of knowledge, which can be made to complete each other. The former (at least in the typical case of equations having a positive sense) contains so to speak a *closed group of precise previsions;* the latter a *group of previsions* that are no longer *a priori* determinate, but *extensible.*

However he who would truly *comprehend a physical theory,* in the fullest sense of the word, ought to go through the process concerned with that theory in both ways, that is to say:

1. He ought to reconstruct inductively the associations and abstractions which have given rise to the concepts, and discriminate the hypotheses contained in them, part of which express the correspondence between certain qualitative differences and certain quantitative relations, and part of which determine these latter relations by means of the fundamental equations of the theory.

2. He ought to develop deductively the *interpretation* of the latter equations, that is find out how they aid us to view the various phenomena as a function of certain determining conditions. This above all requires that one should *complete by means of complementary hypotheses* the knowledge of the quantitative relations and of the corresponding qualitative relations. (We have a simple example of this completion in the fundamental hypothesis of electro-magnetic optics. The same equations are interpreted as corresponding now to actual electro-magnetic oscillations that can be verified by means of a Hertz's resonator or a coherer, now to radiations of light or of heat etc, according to the size of that element which we picture as a "wave-length").

Now to any one who has caught the spirit of this manner of understanding things, it becomes plain that the central point is a *synthetic view of general relations by means of a simplified scheme, in which we make abstraction from the technical details of the experiments and from the developments of the calculus.*

It would be highly desirable that those who deal with problems of theoretic physics should always take care to bring to light this synthetic aspect of the explanations before and independently of the details of the experiments or the calculations. Ultra-analytical methods of treatment will not be an obstacle to the few superior minds which somehow succeed in making a synthesis from them. But perhaps the good wishes which we in our weakness offer may be well received by the greater number.

B—THE MECHANICAL HYPOTHESIS AND THE PHENOMENA OF LIFE.

§ 35. INTRODUCTION.

To view the "mechanical explanation" as a "model," does not destroy the value of the tendency to unify the partial representations of phenomena. Upon the latter tendency the extensive progress of science depends.

All the actual problems arising from the philosophy of mechanics may thus be taken up and examined in their new scientific statement. And when the transcendental concept of the universality of the mechanism has been set aside, the discussion as to the limits which it *might* reach still remains open.

Now as to that which refers to the mechanical explanation in connection with physics, we only need to know up to what point we ought to hold fast by the classic principles of Newton's dynamics. But in a general way we must admit that such a kind of explanation is acceptable, and that at least we can keep the spirit of mechanical determinism. At least we can regard the data of future physical reality as susceptible of prevision, through the quantitative relations which may be determined by certain characteristic conditions of the present or of the past.

The undertaking of following out this order of considerations, by extending the mechanical explanation to the phenomena of life, gives room on the other hand for the discussion of certain prejudicial problems of which we will now speak briefly.

We will first speak of the *possibility* of such an explanation, and then of its *value,* or of the scientific utility which can result from it.

The question of the possibility of furnishing (in the widest sense) a mechanical model for the phenomena of life, leads us,

1. To clear the ground of certain senseless *preliminary objections.*

2. To examine the questions connected with *biological determinism,* and especially the contrast

a. between *psychological determinism* and *free will*; and

b. between *mechanical* and *teleological explanation.*

3. To try to ascertain whether in the phenomena of life the *general principles of physics* are found to be *verified* which follow from the hypothesis of a mechanism.

§ 36. PRELIMINARY OBJECTIONS.

Certain ready-made phrases are especially brought forward as arguments against the mechanical hypothesis in biology, phrases whose vacuity may as well be denounced.

For instance the *spontaneity,* the *change* of every thing that lives, is opposed to the *inertia,* the immutability of matter, and from the antagonism of the images called up by these words, people pretend to get an irreducible contradiction.

Now, if "spontaneity" is here taken to mean "activity" or "possibility of changing through internal conditions," we must say that the notion of an absolutely passive matter does not indeed correspond to the concept gained from a coordination of the various physical relations, in which matter is always manifested as an aggregation of energies. In this sense the view seems far more adequate which holds that everything around us is living and active, save for a difference of degree, in the intensity or in the rapidity of the changes, and in the relative importance of the *internal* and *external factors* for the course of the phenomena.

But the objections we have pointed out, and other similar ones evidently arise from *sentimental* reasons.

Life is not given us merely as an object for representation. The previsions connected with it excite in our minds fear or desire, according to the ends we wish or do not wish to obtain.

Very well. The mechanical hypothesis is incapable of *explaining* the sentiments associated with the representation of living things, or of suggesting a norm for the will. Worse yet, that hypothesis seems to contradict the sentiments to which we have referred by means of a deterministic view of the psychological facts which we shall examine a little later.

What value have these sentimental reasons?

We unhesitatingly answer, none. But still it will be well to justify this answer.

Between knowledge on the one hand, and feeling and volition on the other, this relation exists: To know is independent of sentiment and will, because it implies an hypothetical relation between the will and the following sensation (Chap. II). Sentiment and volition are in their turn independent of knowledge. Science cannot modify the emotions called forth by known objects. It cannot set a standard for the will unless in a relative way, that is in case we are concerned with means, or in case we are dealing with pur-

poses which are recognized as contrasted with other higher purposes, more strongly willed.

Now, for any one who clearly understands these relations, it is absurd to expect that a scientific theory should give an explanation of sentiments, or to claim that the ideas brought together as instruments of prevision should arouse the same sentiments that accompany the facts represented.

In consequence this objection we are discussing against the possibility of the mechanical hypothesis for the phenomena of life, falls to the ground. This hypothesis corresponds to a *contemplative* attitude of mind in the person who is undertaking predictions, and abstracts from the sentiments through which the previsions sought will acquire a *value* in relation to a different (or active) attitude of the same mind.

§ 37. BIOLOGICAL DETERMINISM.

But is prevision possible? Or does the "spontaneity" of all living things hinder it?

This is a positive problem which arises from the differences of representation that we have examined. This is the problem of *determinism in biology.*

The meaning of that determinism really consists in assuming that *determinate previsions,* following the order of coexistence and of causal succession, may refer to the facts of life, and hence that the latter may form the object of a scientific knowledge that is capable of unlimited progress.

Now this deterministic principle cannot be demonstrated *a priori.* It expresses a general presupposition which is equally justified in the various fields of knowledge by the success of the previsions that have been established, that is to say, by the progress of science.

But in this respect there is no difference between the phenomena of life and physical phenomena that cannot be explained on account of the complex conditions of the physiological facts, whether we are dealing with living beings, or merely with their organs and tissues.

The apparent contradiction between diverse phenomena that are produced under apparently *equal* conditions, is resolved by a thorough and discriminating study of those conditions.

Instructive examples in this connection may be found examined and discussed by Claude Bernard in his *Introduction à la médecine expérimentale.* Many other examples could also be added to-day, taken from the more recent advances of biological science.

We will however recall but one extremely instructive example, namely the result of the studies of *malaria* (Laverau, Marchiafava, Golgi, Ross, Grassi, Dionisi, Bignami and Bastianelli), a research which gives a comforting promise that the etiology of maladies will necessarily get beyond that vague obscurity of explanation which has made Molière's physician so famous.

§ 38. PSYCHOLOGICAL DETERMINISM AND FREE WILL.

But in truth biological determinism would not be seriously contested by any one, if from accepting it in its widest sense as a general hypothesis, the consequence did not follow that psychological phenomena also, and in particular our thoughts and our will, may equally well be included in a relation of causal determination.

A feeling of repugnance is connected with this view; a sentiment which would indeed be lessened if we could isolate our own persons from the known world. In fact this sentiment would disappear entirely if it were possible to exclude man or the higher animals from this conclusion, and to limit it merely to the beings further down in the zoological scale, who are so distant from us.

But on the contrary a more mature scientific reflection forbids us to separate in this arbitrary way beings that seem to be connected in a continuous series. Our origin is bound up with that of the vibriones and of the amebas. Since there has been a gradual progress from the lowest forms of life to the higher, since even the phenomena of intelligence have developed insensibly from the animals to man, and since they form a part of life as cause and effect, we must simply make up our minds to a choice: either we must admit biological and psychological determinism in all its extension, or we must deny it and accept the idea of *spontaneity,* in a sense which excludes or limits *a priori* in this domain the *possibility of prevision.*

To settle the difficulty, it helps us to consider first of all the so-called *problem of free will,* by distinguishing in it the *scientific* question which concerns the psychological determinism of human acts, and the *moral* question which is connected with the judgment of those acts.[1] The carrying over of this last order of arguments into the field of science is an undue intrusion, from which we should free ourselves if we wish to gain a scientifically serene view.

[1] Concerning the relations of the question with the recent theories of *penal law* see Mario Calderoni, *I postulati della scienza positiva e il diritto penale.* Florence, Ramella, 1901.

But if this is granted, and if we consider only the facts of other people's wills, all obscurity disappears.

The question "whether the will is *determined* by the antecedents" taken positively, means only this, "whether other people's choices are calculable by one who knows thoroughly the antecedents of the person who is deliberating." Now, provided that we place among these antecedents also the sentiments and the will of the person under discussion, there is no longer any doubt about the affirmative answer.

Direct observations come to the aid of the deterministic thesis, independently of the general motives that confirm physiological determinism. And this takes place with all the more certainty according as we eliminate more and more the causes of error which arise from our incomplete knowledge of the motives which act on the minds of others, and from the complexity of the facts, which may be altered by the most minute variations of these motives.

To the intuition of the *reasonableness* of human actions (that is of their dependence upon motives) which results from previous observations, we add other objective data, for example the *statistical regularity* of social facts, so well brought to light by Quételet etc.

But we need not insist upon these arguments. They certainly suffice, for any one who has freed his mind from any preconceived idea of the moral order, to constitute in their totality an inductive proof of psychological determinism. Or if you prefer, these arguments suffice to justify in this field also the faith in a possible science, no different from that which refers to all other classes of facts, in regard to which we understand the invariable relations of coexistence and succession.

If psychological determinism raises difficulties, they do not arise from reasons of a scientific nature.

Why then have we this repugnance to accepting it?

The answer is clear: Because the deterministic thesis seems to contradict the immediate intuition that every one has of the *freedom of his own will.* Because with the concept of this "liberty" the guiding ideas are connected which stand at the basis of our moral judgments.

It is then important to show that in case our intuitions about voluntary actions are correctly interpreted, *liberty and determinism are not mutually contradictory.*

The thesis of free will, according to the testimony of our consciousness, affirms:

1. that for every man it is possible to *do,* within certain limits, what he has decided upon (physical or external liberty);

2. that it is possible for him even to act up to a certain point, upon the course of his thoughts and sentiments, and hence to determine or modify his further decisions, by inhibiting or enhancing the action of his motives. This "liberty of action" constitutes the so-called *moral liberty* or *internal* liberty.

This has, like the former, a real existence. By it we attain faith in ourselves. In this is the true foundation of our *responsibility,* by reason of which we hold ourselves in the highest degree responsible for those actions which were premeditatedly willed, as a consequence of a mature decision to which we have subordinated a series of acts, and which are hence in connection with the *permanent characteristics* of our *personalities.* On the contrary we think that our responsibility is less for unpremeditated actions. For we impute to this unforeseen element the fact that we are not forearmed against the supervening motive (temptation), by inhibiting its effect upon our wills. Accordingly this responsibility vanishes as it were before our eyes if the action was caused by a strong and unexpected motive.

All this agrees perfectly with the common sense of all men, and as has been said, it is necessary for practical life, which requires that we should have confidence in our own wills.

But in all this we find nothing contrary to the deterministic thesis that voluntary actions are capable of prediction. Apparent contradictions can be obtained only from a vicious method of reasoning affected by transcendentalism.

Some element of transcendentalism may here be introduced in two ways.

In the first place by applying to the freedom of the will an old argument of Locke and Leibniz, which had indeed a different meaning originally.

If we admit a second degree of volition, consisting of "willing to will," we ought to admit a third and fourth degree, and so on to infinity.

From this infinity certain people derive a transcendental concept of the will which *determines itself,* whence the *arbitrium indifferentiae* through which prevision becomes impossible. But this argument rests upon an equivocation.

When we say that we can "will to will," the will taken as the subject is not the same thing as the will forming the object of the volition. The volition of the second degree does not then imply the non-sense of a volition which determines *itself,* but rather the psychological fact that an act of the will can be subordinated to another one preceding it. Now, evidently this subordination (which indeed is partly subject to external contingencies) cannot go beyond a finite number of degrees, the requisite time being given for the act of volition. It is not then even necessary to discuss the preceding dialectic instrument (borrowed from natural theology) and to point out that we cannot without contradiction argue from an *infinite* series of causes, to one *first* cause which shall be its own cause.

Another way of arguing, which looks more profound, viciously introduces a transcendental conception of human personality. The statement "I can do and will just as I please, be it even in opposition to the motives acting upon my determinations"[1] expresses a notion of the person who acts as if he were independent of the series of his own volitions.

Now, if we attempt to attach any intelligible meaninng to such a statement, there is but one way of understanding it. The ego who is pleased to do and to will, is nothing but a continuous expression of that same will, which subordinates the transitory and accidental motives, to those that have previously been fixed upon as permanent.

The *moral personality* of a man is identified precisely with this continuous volition, resulting from the organic factors and from elements due to the external environment. No doubt of this statement arises when we are talking of other people. But when we are referring to ourselves, we are easily deceived by our habit of dividing our ego, taking it now as subject and again as object. That is to say, we confound ourselves as the subject of the action, with the notion which we form of ourselves.

Once we consider our ego as the object of a representation, we are easily convinced that we cannot attribute to that representation, a content that goes beyond the collective idea of that which is relatively permanent in our volitions.

For if we set up against this reality, regarded as *apparent* and *relative,* an *absolute substance* or *soul,* in which we will have the

[1] Cf. the lively dispute described by A. Herzen, *Le cerveau et l'activité cérébrale.* Paris, F. B. Baillière, 1887.

ethical personality reside, we fall into one of those infinite processes of definition whose emptiness we have already shown.

Unfortunately this transcendental view of the mind has, until now, predominated in the controversy as to free will—a controversy whose history is connected with that of the religious problem of predestination and grace.

This aspect of the question still governed the thought of Leibniz in his "Theodiceé."

Hence the philosopher, although opposing to Spinoza's external fatalism a clear vision of an internal psychological determinism, was forced to attribute to *liberty,* understood as the *spontaneity of an intelligent being,* a transcendental and metaphysical sense.

Precisely this sense is shown, with great clearness in the further evolution which determinism has received in the philosophy of Kant: phenomenal determinism contrasted with noumenal freedom; that is free will in itself, inasmuch as we are considering the intelligible cause of volition (?!), but determined according to immutable laws in its manifestations. The natural development of this distinction finally leads to the doctrine of Schopenhauer. The will, transcendentally understood as outside of its acts, outside of space and time, takes the place of *substance,* or of *things in themselves,* an idea whose anthropomorphic foundation we have already set forth.

These ultimate conclusions of the classic philosophy are in our opinion very instructive. Because from them comes, on the one hand, the recognition of psychological determinism, in its human and positive sense; on the other hand the demonstration that the problem of free will is of no importance to practical reason, unless in so far as a transcendental foundation may be considered necessary for morality.

But, although the transcendental idea of a will and of an ethical person independent of their manifestations may be considered as without sense, yet it clearly follows that the thesis of free will is not contradictory to determinism.

Both theses keep their full value in their respective fields. The possibility of an unlimited extension of the science of psychology does not deprive us of our *judgment* of human actions, inasmuch as they are *willed* or *not willed* in connection with given *ends.*

It is true that the thesis of determinism has had a discouraging influence upon some feeble wills, owing to a false interpretation. Stuart Mill himself experienced such an effect, and recounts in a

significant passage how he was freed from this influence: "The theory which I now for the first time understood in its true sense, no longer seemed discouraging, and besides the relief that came to my own mind, I ceased to suffer from that burden which is so heavy for one who aims to reform other men's opinions, of believing one doctrine to be true, and its opposite morally beneficent."

But similar effects of erroneous interpretation may be offset by the advantages that a more powerful will may gain from the deterministic theory when rightly understood. For the exact knowledge of the relative weight of the motives that urge us on to our deliberations, the adequate judgment of the way in which we are able to react to stimulations or to inhibit their effects, teach us to strengthen our own wills, by so disciplining ourselves as to secure the triumph of our permanent personalities over the changeable influences of the external world.

§ 39. PHYSICISM.

The ground being cleared of the psychological-moral difficulties, let us return to the scientific consideration of the phenomena of life.

Determinism implies in the general sense the more extended possibility of discovering in the series of vital phenomena the invariable relations of coexistence and succession. The mechanical hypothesis adds a limiting idea, inasmuch as it suggests the possibility of selecting as determining circumstances, data having a *physical* significance, apart from life.

This particular way of understanding biological determinism is called physicism. It may be rendered more precise and concrete by the following requirements:

1. That the *biological explanation* ought to represent the phenomenon in question as the consequence or the invariable connection of physico-chemical data (heat, electricity, motion, etc.) occurring together in certain quantitative relations.

2. That hence the aggregate of the physico-chemical phenomena give rise to a condition in which a phenomenon is determined that is analogous to the one in question, is so even *apart from life*.

And whenever *physicism* is considered not so much as a remote aspiration, as in its practical efficiency in relation to the actual state of science, then the following hypotheses are added implicitly:

3. That the analysis of the physical causes of the phenomena of life may be reduced to not too great a number of elements.

4. That the noteworthy conditions of which we should take account in physical causality, are not changed when we consider the same phenomena in the realm of life.

Now the results of the studies of the last fifty years precisely contradict this latter opinion. Therefore these results weaken physicism, in the practical sense that has been defined in the preceding statements, apart from a belief in an extremely remote progress.

In fact it follows that *we cannot deal with biological physics by abstracting from the essential condition of life.*

Only a few general physical relations, persisting through all varieties of conditions, are found to be verified without change in the realm of biology, as for example the conservation of matter or of energy. But among the less extended laws that refer to diffusion or osmose or electric conductivity etc. we meet at every step with exceptions and apparent contradictions.

Here we have the *torpedo*, a living Leyden jar, the electric charging of which takes place under paradoxical circumstances. While the functioning of an electrical machine is so easily hindered by the moisture of the insulator, here we see a charge which is not lost in the surrounding water with which however the tissues of the animal are saturated!

The importance of such well certified facts cannot be lessened or denied even though we are dealing with rare exceptions. But we ought all the more to take account of these facts, granted their number and their striking frequency, and granted that we are thus dealing with differences which already appear even in the lowest orders of life. The general physiology of the cell, as Max Verworn has presented it in his excellent treatise, and the physics of the tissues, afford many and characteristic examples of this fact.

We will cite just one example. The living cells of the bladder hinder the diffusion of the water. Yet we do not find any impermeable membrane which explains this obstacle! We can only say that a moist tissue prevents the passage of water *by virtue of being alive,* because it loses this property as soon as death has taken place.

Phenomena of this sort may always be reconciled with a theoretical physicism by the supposition of relations too complex to be analyzed into their hypothetical elements. But they suggest a positive attitude of thought which renounces at least provisionally an *analytical explanation,* and resorts to a *synthetic explanation.* The *fact of life* may be taken as a fundamental condition. Thus we

may express the average result of the controlling circumstances that are united.

We may now take all the noteworthy circumstances that appear in connection with life, without searching into their final reduction. And instead of arbitrarily simplifying *a priori* the physical determinism in living media, we may try to find out the characteristic differences that are fitted to give in their totality a *positive definition of life.*

For this purpose we refer, amongst other things, to the admirable researches of our own G. Galeotti, from which it appears that the living protoplasm hinders the diffusion of certain substances, that in general it forbids the establishment of an osmotic equilibrium, and in certain cases it offers an especial resistance to the ions moved by electro-motor forces.

§ 40. THE TELEOLOGICAL EXPLANATION.

In the preceding discussion we have especially referred to physical phenomena as developed in a living medium. From this we see all the more clearly the need of a synthetic explanation of the especially physiological phenomena. Our critique shows that such a method of explanation is not incompatible with the mechanical hypothesis and with the theoretic physicism which is derived from it, provided we admit that the latter has no practical significance as a guide for actual research.

But we are now trying to ascertain whether at least theoretical physicism does not impose certain limits to the explanation accepted in physiology, and especially whether it does not imply that any *teleological explanation* is to be regarded as devoid of meaning.

We will first state the fact from which this problem originates.

Most facts of our physiological knowledge take a form which is the inverse of that in which we represent a connection between cause and effect. We may meet such conditions in almost any class of the phenomena of life, and not only in the synthetic study of living beings but also in the physiology of the tissues.

If for example we are trying to understand what substances and tissues prevent diffusion, we are led to appeal to the *use* or the *need* that the protoplasm has for reacting in one way rather than another, as if the antecedent could be determined by the consequent rather than *vice versa.* This same idea also enters into the so-called *law of cellular reaction,* which seems to place the *concept*

of the *end* or *purpose* at the *basis* of every *explanation* in the science of life.

But such a way of explaining the facts is very repugnant to those who take the physical sciences for their model. It would seem at first sight that an irreconcilable contrast exists between the teleological view and physicism, even when theoretically understood. And from this circumstance the physicists make the deduction that all teleological explanations ought to be strictly eliminated as devoid of sense.

We will presently try to see whether the contrast pointed out can be reconciled. Meanwhile we observe that in practice we should not be able to follow so simple a rule.

In fact those who hold by that rule are then unable to substitute anything else for the numerous discriminations of cases without apparent connection, cases which the physiologist unifies by judging of certain differences *in relation to the purposes of life.*

The physicists will answer that the latter are *verbal explanations without sense or value,* a veil thrown over our ignorance, which we may well remove without regret, even if we have nothing else to put in its place.

This would indeed be the case if the teleological expressions were to be taken in the sense of a mystic vitalism, by confounding the conceptual point of view of knowledge and the active point of view of the will.

But a positive meaning can be found in the teleological expressions, and so from this point of view we disagree with the criticism of the physicists!

It is not true that usefulness or injury to a living creature are senseless expressions. For they contain a fact which can be experimentally verified.

It is not true that those explanations in which we appeal to the idea that something serves a purpose of life are without value. For by appealing to similar determining conditions we obtain *significant previsions* in various groups of phenomena.

And it is for this reason that notwithstanding all objections, the teleological explanation remains in the science of life.

It only remains to give a fitting interpretation of it, and to show that this does not really imply an irreconcilable disagreement with the spirit of mechanical determinism.

Let us distinguish two types of explanation into which the idea of purpose enters in different ways:

1. The psychological explanation in which a certain purpose is regarded as the motive determining the voluntary action of a living being.

2. The more strictly biological explanation in which the structure of an organ is considered in relation to the function which it is to perform. Or in general the effort is to represent certain biological characteristics or certain reactions of the protoplasm etc. as answering for certain useful ends.

The first case can be easily connected with the customary determinism, because we recognize as the cause of the action not actually the end proposed, but its representation, which results from recalling and associating in different ways the previous sensations.

It is difficult to reduce the second case to the first one, unless we resort to the hypothesis of a Providence, which determines actual reality as subordinated to the purpose of a future, just as we ourselves make a machine capable of exercising certain functions.

Now, whoever will consider such an hypothesis apart from its moral and religious value, so as to judge it only from the standpoint of its usefulness for knowledge, must admit that it has no scientific value, since it does not enable us to make previsions, or else it offers us previsions which are found to be indifferently true or false, without providing us any possible criterion of choice.

It remains to be seen whether the biological explanations of the second type can be brought to agree with the idea which we form of determinism, independently of the hypothesis of a providential intervention.

This agreement is actually possible if we interpret the teleological explanation in a positive sense, as expressing a *first degree of knowledge.*

Instead of comparing this explanation to that at which we aim in the highest development of physics, where we proceed from known causes to unknown effects, we will compare it to the explanation which has its place in physics itself as the first step of inductive research, when we are trying to shape out hypotheses from preliminary observations, which is to say, from the known effect we try to go back to its causes.

The problems of physics also may assume in this sense a teleological aspect. It will suffice to take as we need them certain examples from mechanics and from astronomy.

We can imagine various kinds of mechanical systems which are in a certain state of stable equilibrium. Every little motion

produced by any kind of perturbation generates forces that bring the system back to its original position. In similar cases the stability being given as a condition to satisfy, the problem is to seek the forces which explain its existence.

In an extended sense of the word, even the planetary system has a stability, practically verified within the limits of observation—a thing which confronts the astronomers as a problem.

And this problem was solved, not by Newton, who suspected that the mutual attractions of the planets must destroy the permanent equilibrium of the system, but by Laplace who found out how the effect of the perturbations on the elements of the orbit tends to be compensated periodically, apart from a small secular variation. Thus was reestablished the superb harmony of law which Newton himself had failed to grasp. The hypothesis of the intervention of Providence for the sake of this harmony proved unnecessary.

Another analogous question is that of ascertaining the stability of the ring of Saturn. This is a fact which we may state as the result of unknown causes, which we are concerned with finding. Now if we make the simplest hypothesis that the ring is made of solid matter, and then estimate the forces of attraction that are exerted between the various parts of the body and the action of the mass of Saturn upon it, a stable equilibrium does not result. For this very reason we are led to discuss different hypotheses, searching for an arrangement which shall correspond to the conditions given.

These conditions, taken as it were as an end to be satisfied, exist in advance of the unknown disposition of the mass of the ring, only in the order of our knowledge, which appears to us the inverse of what we conceive to be the real order.

After observing these examples we can better understand the manner of explanation used in biology, in which we speak of an organ as serving for a function, or of a manner of reacting in relation to some useful end. We can transform these expressions in such a way as to get rid of all appearance of being in opposition to physical determinism. We need in fact to remember that, amongst the manifold elements of the fact, life appears to us synthetically as given *before* its supposed factors, not indeed in reality but in the order of our knowledge. From the fact that protoplasm has life we infer as a highly probable hypothesis that it has a certain faculty of reacting in a useful manner, at least so long as the stimuli concerned are not such as to cause death etc.

Such a method of explanation would be perfect and could be

converted into an explanation of the physical type, if in working back from the given effect to its causes we could succeed in determining the whole group of noteworthy causes, whose total result would yield the experimental reproduction of the phenomenon.

This result has not as yet been obtained in case of the properly physiological explanations, but nevertheless they contain a degree of positive knowledge which the foregoing observations bring to light.

In fact we do succeed in working our way back from the effects, which we represent as the "ends" of life, to some of the proximate causes which unite to produce them, and from these we pass gradually to the more hidden concomitant conditions and to the more distant causes.

Only here the chain is more complicated and longer, and it is not given to us to invert the series, just as it is not given to the chemist to explain a painting of Raphael's by an analysis of its colors!

§ 41. LIFE AND THE PRINCIPLES OF THERMO-DYNAMICS.

Thus far we have not found any irreconcilable contradiction between the mechanical hypothesis and the science of life. In order to carry our examination further, we must seek to verify for the life process, those principles of physics that result from more general mechanical hypotheses. We will pass over the *conservation of matter* which holds for living beings, and speak of the principles of energetics, and in particular of those of thermo-dynamics.

The principle of the *conservation of energy* is verified in case of the phenomena of life. Outside of this nothing positive is known.

It would be very interesting to examine more deeply the second principle of *thermo-dynamics* in its relation to life.

According to the mechanical hypothesis this second principle appears as the result of an average condition in a disorganized system. But in the case of a living being the application of the postulate of an average state raises grave doubts. For what, in fine, does that postulate express? It expresses in numerical form the hypothesis of a certain statistical regularity of a great number of phenomena whose differences cannot be explained by systematic causes. Statistical regularity well suits the condition of something which appears homogeneous, or tends to a certain homogeneity. But the special character of living matter is, on the contrary, a thorough-going heterogeneity. The thought that living matter involves an organization of movement would appear very natural.

Furthermore, it seems difficult to reconcile such an hypothesis with the persistence of life. Side by side with an organized kinetic system endless disorganized systems seem possible, so that the passage from the one to the others corresponds to infinitesimal variations. For this reason we should not be able to explain the persistence of an organized state.

As we see, the mechanical hypothesis alone would not perhaps suffice for deducing the validity of Clausius's postulate for the phenomena of life, though it renders it probable.

Now, however we value these theoretical reasons, the answer can only come from experience. The thermo-dynamic investigation of the physiological processes ought to solve the problem whether *the postulate of Clausius also holds good for the phenomena of life.*

The question seems more accessible from the side of vegetable life; and certain results as to the intervention of light in chemical transformations which accompany such processes, lead us to expect an affirmative answer.

§ 42. THE MECHANICAL HYPOTHESIS AND THE PROBLEMS OF EVOLUTION.

From the foregoing analysis it appears that, at least in its actual state of development, *the sciences of life do not in any way contradict the mechanical hypothesis.*

But this conclusion is purely negative, and the question now is how to judge of the *positive value* of the hypothesis itself.

Our thesis is that the *mechanical hypothesis may be regarded as of no importance for the progress of biology.*

First of all, the discussion in § 39 showed that the view of a theoretic physicism suggested by the mechanical model was not to be interpreted as a practical guide for research. The remote possibility of such a model has in this respect no influence over the type of the explanation in case of the phenomena of life.

In the second place it is easy to verify the fact that the mechanical hypothesis in its undetermined generality is incapable of solving the general problems of biology in one way rather than in another, as for example to support one or another of the tendencies of thought, *epigenetic* or preformistic, which undertake the task of explaining *organic evolution.*

We can imagine various series of mechanical models, endowed

within certain limits with a certain permanent regulated order, so that they would give rise to the following cases:

1. Every mechanism passes periodically through the same phases except for an accidental variation, which is statistically compensated by the various mechanisms of the series. But external forces impart to every mechanism a variation in a determinate direction, producing an evolution of the medium type of the series.

2. The statistical periodicity of the mechanisms of the series still holds good. But the external forces that have been added give to the medium type a determinate evolution, not because they are capable of very greatly modifying the development of the single mechanism, but because they bring about a choice tending to destroy the permanent order of those mechanisms whose variations occur in a certain direction.

3. The mechanisms of the series show a quasi-periodicity. The medium type to be sure is repeated quasi periodically, but above the rapid periodic variation there is superposed a slow variation in a determinate direction.

So far good. Apart from the difficulty of defining these models, we can easily see that they show the possibility of reconciling the three species of fundamental hypotheses that have been called upon to explain the evolution of living beings, and the different weight which is to be attributed to the internal and external causes of variation.[1] Such are the following hypotheses:

1. Individual hereditary variations proceed in an indifferent direction so that from the internal factors there results a rigid statistical periodicity of characteristics, that is to say the constancy of the species. But *environment modifies living creatures,* and the characteristics acquired by adaptation are transmitted by heredity to later generations (Lamarck's hypothesis).

2. Individual internal variations would also give as a result a statistical compensation, that is to say the fixity of the medium type, but owing to the struggle for existence the *environment selects the useful variations,* which are transmitted further by heredity in

[1] It is hardly necessary to point out that, quite apart from opinions regarding its factors, the *hypothesis of evolution* is generally accepted on the basis of the following reasons:

1. The direct proofs furnished by paleontology.
2. The indirect proofs gained from botanical and zoological geography.
3. Evolution explains a series of anatomical and zoological facts, for example, rudimentary organs etc.
4. Evolution gives us a synthetic criterion that serves as a guide for scientific research, and therefore is of practical value for the progress of the natural sciences to-day.

a way that gives to the evolution of the species a determinate direction (Darwin's hypothesis).

3. Hereditary internal variations are not strictly balanced so as to form a medium type, but proceed in a determinate direction (Eimer's orthogenesis). That is to say we have a development of the species that is perfectly analogous to the ontogenetic development. (Indeed Weismann's continuity of the germ protoplasm gives the notion that the species is like a long-lived individual. In this hypothesis the environment has a secondary and somewhat negative function. Natural selection commonly acts as a means of preserving the species (Pfeffer), whose efficiency is indirectly proved by certain retrogressions, like that of blind moles (*Panmixia*).

§ 43. THE IRRELEVANCY OF THE MECHANICAL EXPLANATION IN BIOLOGY.

The preceding considerations show that the mechanical hypothesis, taken in its generic indeterminateness, is of no importance to the progress of the sciences of life.

But more than this. Apart from the extreme difficulty of investigating the hypothetical mechanism for every given order of the phenomena of life, there is room to inquire what value there might be for the questions with which we are concerned, in the particular description of that mechanism and in the knowledge of the quantitative relations connected with it.

We have already pointed out what would be the actual value of the mechanical explanation for physical phenomena.

The difficulties in the way of a complete explanation are divided into two parts:

1. In the one, our concern is to fix, by means of a fundamental mechanical hypothesis, the relations between certain data, abstractly taken as *quantities*.

2. In the other, we are concerned with *interpreting* these relations by means of *complementary hypotheses*, adding *qualitative previsions* to those which are directly quantitative.

The mechanism of phenomena does not exhaust our knowledge of them even in those cases in which it can be somewhat precisely described. The vibrations of a cord which the mathematician represents as an optical fact, by means of Fourier's series, contain relations of harmony, which may well be associated with the form of the series, but which always form an *interpretation or association added* to the visual sensations symbolized in it.

Now the solution of problem 1 is of a relative importance that is all the greater according as the greater part of the questions asked refer to *measures*. But on the contrary, the so-called determining equations that express the fundamental mechanical hypothesis, become the less significant as an increasing use is made of the complementary hypotheses, which aid in their interpretation.

Very good. If we classify the physical problems in a series in which the *interest* of the mechanical explanation continues to decrease, *the problems of life will appear as a limiting case in such a series.*

The mechanical explanation then appears to be irrelevant in this field of science.

It is interesting to compare the preceding conclusion with that to which Du Bois Reymond was led, in treating of the mechanical problem of life.

The impossibility of exhausting the biological fact by the mechanical explanation appeared clear to the illustrious philosopher. But he interpreted this impossibility in an agnostic sense!

That merely resulted from his having accepted an inadequate view of the scientific explanation, forced into the arbitrary limits of quantitative metaphysics.

The problem is insoluble because it is ill stated. And the so-called agnostic conclusion is reduced to the recognition of the fact that the relations forming the object of the biological previsions, are less reducible to quantitative relations than are the physical relations, can be understood as quantitative relations, even if we admit the possibility of a mechanical model.

§ 44. CONCLUSION.

Our conclusion is that, in the actual state of our knowledge, the *mechanical hypothesis does not appear to be incompatible with the phenomena of life, but it is unimportant for the study of these phenomena.*

That hypothesis had, however, an important office in the past, in the first place because it clearly affirmed that knowledge is independent of sentiment (cf. § 36), and in the second place it played its part in unifying scientific views. Through this hidden mechanism we have learned to find in life also a proper object of science, and to understand the intimate connection of the methods which have been employed in the separate departments of learning, merely

through the need of a division of labor, while they are once more united in case of a concrete prevision, that is to say, united in reality.

In order to give an adequate account of the result obtained in this way, we need to go back to the time of Descartes, when Harvey's ideas as to the circulation of the blood met with an obstacle in the current prejudices about the *power of the soul,* to which reference was made in connection with every vital process.

It was an immense progress to have made out that there is a unique physical order, which extends unchecked to the phenomena of life.

And it was a magnificent approximate generalization which led to conceiving the variety of the conditions as subject to simple and uniform physical laws. The conditions themselves form the topic of a later corrective view which may be neglected in a first approximation.

From all this there follows a just appreciation of the unifying tendency of mechanism. It gives a representation of the early development of the science of life. Only from a more advanced point of view it appears that the unified view is constructed with somewhat restricted and onesided standards, because it accepts as general and perfect the type of a more developed science, and attempts to subordinate all other forms of knowledge to this in too rigid a way.

Hence the views of the philosophy of mechanics are modified by the progress of biology, which, reacting in its turn upon physics, prepares us to understand a higher unification, that is a more perfect and general type of science, which shall contain the various special forms which technical requirements tend to keep apart.

This higher type, which is adequate to the various domains of knowledge, is not a rigid scheme of dogmatic arrangement, neither is it a stratification of knowledge in which facts are simply piled upon facts. It is a *process of association and abstraction* which works upon the sense data by forming the representative concepts of the real invariants. It is a development of composition and decomposition, which is unfolded in two phases, inductive and deductive, and results in a *progressive extension and correction of our previsions.*

As in the various forms of the evolution of life, the development indicated may be slower and longer, or more rapid and shorter; the inductive and deductive phases may be designated as distinct moments in the scientific process, as we can clearly observe

in physics, or on the other hand those phases may be interwoven and, as it were, blended as in the case of biology.

Now the comparison is interesting from two reciprocal aspects. Only in physics are we able to gain a view of the degree of perfection toward which the vital sciences may tend in the future. But the enormous complication of the latter, and the instability which that complication imparts to the explanations of biology, make clearer to us the advance of science, which in relation to certain classes of physical phenomena becomes visible only during centuries. In this way we come to understand that *exact knowledge* is only a *good approximation*, which may be considered satisfactory only in relation to a certain circle of previsions, but in relation to a wider circle it ought sooner or later to be submitted to correction, in which the complicating conditions that have been neglected must be taken into account.

Let us recall an example of arguments that have been already discussed.

The dynamics of Galileo and Newton seems now, after two hundred years, to require a corrective hypothesis in this sense: The abstract distinction of certain data that determine motion, *internal* and *external* to the moving body, are completed by a postulate of *inheritance* (determining influence of past motion) or of the *solidarity* of the field of motion (§ 33).

Even the very words by which we designate the corrective hypothesis allude to the analogy with certain classes of determining conditions which appear in life. The biologists who study the evolution of organic species, actually tend to separate (as in motion) the *internal* and *external* factors of the variation (preformism and epigenesis), and they continually refer to the idea of heredity and to the solidarity of the organs. In the more complicated field of research, as we now see, the synthetic point of view is presented first and is brought forward more prominently. In an apparently simpler science this point of view is brought forward only when a greater degree of precision has become necessary.

Thus we may say that the *development of the sciences of life appears as an abbreviated recapitulation of that of the physical sciences!*

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